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THE OBSERVATORY,

A

MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

E. W. MAUNDER, F.R.A.S.

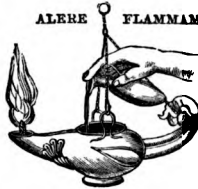
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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 69.

JANUARY 1.

1883.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1882, December 8.

E. J. STONE, M.A., F.R.S., *President*, in the Chair.

Secretaries: J. W. L. GLAISHER, M.A., F.R.S., and
E. B. KNOBEL.

THE Minutes of the last Meeting were read and confirmed.

Mr. Knobel announced that 54 presents had been received since the last meeting of the Society.

The thanks of the Meeting were unanimously voted to the respective donors.

On the motion of *Mr. Penrose*, Messrs. Chambers and Lecky, and on the motion of *Capt. Noble*, Mr. Penrose, were appointed Auditors.

The President. The Society has sustained a serious loss in the death of Professor Challis, who quietly fell to his rest on Sunday last. His removal cannot be described as premature, for he had reached the advanced age of 78 years. Twenty years ago he was one of the most prominent members of the Society; but since that time he has not taken any very active part in its work. For 48 years, I believe, he held the position of Plumian Professor in the University of Cambridge. For a great many years he was Director of the Cambridge Observatory, which, during the whole of that time, was in a perfectly efficient and active state; in fact the activity became at length so great that Prof. Challis, in a most disinterested spirit, resigned the Directorship of his Observatory in order that another might discharge the active duties of the position, while he brought up the arrears in the reductions and made the results available to astronomers. In this way he has done much for the practical appliances of our science in those branches which do not attract immediate attention, but which, I think, are quite as necessary for the real advancement of Astronomy as other branches which

command more direct notice. (Hear, hear.) Prof. Challis endeared himself to all, and particularly to the young men who were fortunate enough to come in contact with him; and he has in that way done perhaps as much to advance the interests of science as by his more direct scientific work. (Hear, hear.) I am sure it is the feeling of all who have known him that, although we have no right to say that he was prematurely removed from us, yet the Society's loss in his death has been very great. (Hear, hear.)

Mr. Knobel read a communication from *Mr. Gill* on "Photographs of the Great Comet (b) 1882, obtained at the Royal Observatory, Cape of Good Hope." The photographs were obtained by *Mr. E. H. Allis*, of Mowbray, with a Dallmeyer rapid portrait-lens of $2\frac{1}{2}$ inches aperture and 11 inches focal length, mounted on the declination-axis of one of the observatory equatorials. In the third photograph the stars are slightly elongated. The first photograph was taken on Oct. 18 with an exposure of 30 minutes, the second on Oct. 20 with an hour's exposure, the last on Oct. 21 in 40 minutes. The first photograph shows a curious faint envelope extending 2° beyond the head, which is again shown, but less distinctly, in the third. The sharp termination of the tail, some twenty degrees from the head, and the luminous tuft in the centre of the extremity of the tail, remarked by so many observers, are very distinctly presented. The photographs are also interesting, as showing a marked difference between the photographic and visual magnitudes of many of the stars.

Mr. Ellery also sent a photograph of the comet taken on Oct. 10 by a local photographer at Melbourne. The camera in this instance was not equatorially mounted, and the nucleus of the comet is therefore very much elongated.

An extract from a letter to the Rev. T. W. Webb, by *Mr. J. T. Stevenson*, on "Observations of the Great Comet (b) 1882 made in New Zealand," was then read. *Mr. Stevenson* gave a detailed description of the appearance of the comet, the most noteworthy feature being a faint "tail," which extended from the comet towards the Sun. The axis of this tail was not quite parallel with the axis of the brighter tail extending away from the Sun.

Mr. Brett. I cannot but doubt the accuracy of your reading of *Mr. Stevenson's* statement, for I never before heard of the tail of a comet being dark. Another curious remark which he makes is that it was in "perihelion with the Sun." He speaks as if it might be in perihelion with the Moon. What can he mean?

Mr. Knobel. I am not prepared to say what he means.

Mr. Brett. There is evidently a mistake somewhere.

The President. I think *Mr. Brett* is rather hard upon *Mr. Stevenson*. He says here:—"There was also seen a fainter tail extending from the northern part of the head and reaching as far as the brighter one. With the naked eye the northern part of this tail seemed to be darker than the other parts." He means, I suppose, that it was relatively not so bright.

Mr. Brett. If people cannot write accurately, probably they cannot observe accurately.

Mr. Knobel read also an extract from a letter of *Dr. Schmidt* of Athens. Accompanying the letter was a drawing of the Great Comet and of the companion Comet which he had observed on Oct. 9, 10, and 11. The real comet stood in a sort of nebulous tube, which tube stood in an outer circle of which *Dr. Schmidt* could only perceive traces. The companion object, which was only seen up to Oct. 12, was an irregularly shaped patch of nebulous matter without any defined nucleus—according to *Dr. Schmidt* not a comet, but probably a portion of the corona torn away from the Sun by the comet at its perihelion passage.

Mr. Christie. This is simply an extract from a private letter of *Dr. Schmidt's* to a friend of his in England who sent it on to me. I thought it would be of sufficient interest, being accompanied by a sketch of the appearance of the comet and its companion, to be presented to the Society. Of course *Dr. Schmidt* was not aware of the more recent and more exact calculations of the orbit of the comet.

Major Herschel. Has the writer given permission for the publication of the letter?

Mr. Christie. It was communicated to me by the person to whom it was addressed. It was not presented to the Society for publication; but I thought it would be of interest to the Society to possess the original drawing by *Dr. Schmidt*, so that it might be available for reference, not with a view to publication.

Mr. Brett. The drawing has been published in the '*Astronomische Nachrichten.*'

Mr. Knobel. It is not entered as a paper received, but is merely brought forward collaterally with the other papers.

Mr. Brett. I should like to say a word on the drawing of *Dr. Schmidt's*. He gives a certain curvature to the tail. Now, if there is anything important about a comet it is the curvature of the tail. There is in the room a photograph taken on the 10th of October, which gives a curvature so different from this of *Dr. Schmidt's* as to cast very grave doubts on the whole thing. You are going to admit photographs in evidence, and it is well these doubts should be cleared up. There are photographs taken by *Mr. Gill* which are remarkable productions, and one is extremely beautiful. As I criticised at the last meeting my own drawing, I hope you will allow me to do the same with other people's. My own drawing showed a very different curvature to that of *Dr. Schmidt's*; but it might be objected that it was not made on the same date, and that the tail had suffered some change in the interval. Happily there is a photograph in the room taken on the 10th of October, at Melbourne, the very day of *Dr. Schmidt's* observation, which corroborates my own drawing satisfactorily; so that it is clear that the direction of the tail had not altered. One other remark on the one of *Mr. Gill's* photographs which is undoubtedly the best, and which is a

very beautiful production ; it seems to me as like my drawing as you possibly could wish [Laughter and Hear, hear], but it omits the elliptical form of the extremity of the tail, which, I said at the last Meeting, I thought an extremely important feature. I shall be glad if any other Fellow can throw any light upon that point as to whether it is an optical illusion or not.

Mr. Common stated that he had seen the projection of the tail towards the Sun mentioned in Mr. Stevenson's letter. In October the head of the comet appeared like the nebula in Andromeda, an elongated nebulous mass, with a more luminous patch near the middle. Later there was a remarkable dark space in front of the comet, with an extension of light towards the Sun on one side of it. His observation made with his large telescope had been corroborated by the sketch made at Washington, showing the same ill-defined extension of the tail in the direction of the Sun.

Mr. Brett. We should generally call that the head.

Mr. Common. I cannot exactly tell what to call the head. After my first observation I found a spot get brighter further down towards the tail, and later still it became brighter at yet another point, so that it was difficult to call any one point of the comet the nucleus.

The President. Is there not a possibility of there being several nuclei instead of one? because it appears that these observations made at the Cape would be easily understood if, instead of a single solid body with a certain amount of vapour surrounding it, there were several isolated bodies. In that case there would be a certain amount of dispersed light. Looking at the evidence as it stands, without expressing a strong opinion, it seems to me as if we have something in accordance with the theory that a comet is a collection of meteors, and that instead of consisting of one body there may be a collection of separate bodies forming what may be properly called the nucleus of the comet.

Mr. Common. I can speak with certainty as to the appearance of the comet on the 17th. It was then a bright planetary disk, and there was a very short tail with parallel sides. I did not close the wires of the micrometer to measure the nucleus ; but I compared it with a spot on the Sun ; and the size of the spot as given by the Astronomer Royal was 47" to 50" in diameter. This disagrees with the observation of Mr. Finlay, who saw the comet disappear at the Sun's limb, and who gave the diameter as 5". At this time the coma was nearly as bright as the nucleus. The nucleus was like a planetary disk, intensely white and bright. When I saw it again at the latter end of October it was a hazy object, as Mr. Stevenson describes it, like a candle seen at a distance through a fog. After that it got much longer and very much brighter, with two, if not more, decided nuclei in it. In the beginning of November the two nuclei were separated by a distinct dark space. On one occasion I thought it had become triple ; but there can be no doubt about the two nuclei.

Mr. Knobel. We have received several communications from Mr. Eddie, of Grahamstown, Cape Colony, on the subject of this comet. As they are merely extracts from newspapers, we are not able to bring them before the Society; but he distinctly records two nuclei in the head of the comet, as described by Mr. Common. Before the next Meeting we hope we shall have communications which may be read to the Society.

Mr. Chambers. If I rightly understood one of the remarks made by Mr. Brett, he seemed to throw some doubts on the good faith of Dr. Schmidt, which ought not to pass without a protest on the part of the Society. Dr. Schmidt is an observer of world-wide experience and reputation. (Hear, hear.) And when we consider how notorious are the differences in the meteorological condition of the air which prevail from time to time, we ought to hesitate before we impute bad faith to a drawing submitted to us, because we are not able all at once to reconcile it with other productions. (Hear, hear.)

Mr. Waters. I believe the difference between the drawing and the photograph may be accounted for by the difference between the instruments employed. I believe Mr. Gill's photograph was taken with a camera equatorially mounted, whereas the Melbourne drawing was taken with a fixed camera. Consequently the comet will appear considerably elongated in the Melbourne photograph.

The President. I do not think Mr. Brett had any intention of arraigning Dr. Schmidt's good faith.

Mr. Brett. I had no such idea. (Hear, hear.)

Mr. De la Rue. I have had an opportunity of making three naked-eye observations of the present comet; and my impression was that it had the apparent size of Donati's in 1858, but was not nearly so bright. We have now before us several very successful and beautiful photographs of this year's comet; and what I wish to call attention to is the fact that in 1858 I endeavoured to photograph Donati's Comet with my 13-inch reflector, and that during the interval a very remarkable advance has been made in the processes of photography. By reference to vol. xxx. of the Memoirs of this Society, I find that I then made two attempts to obtain photographs of the comet. After the last attempt made on the 2nd of October, in order to ascertain whether the chemicals were in a sensitive condition, I remained in the observatory, and tested them early on the following morning, and a successful photograph of the waning Moon, within a few days of a new Moon, was taken in 30 seconds, when she had attained the same altitude as the comet on the previous evening. In 1861 I attempted to photograph the comet of that year with a camera fastened to my reflector, and again failed.

The President. What was the length of the exposure on the comet?

Mr. De la Rue. Five minutes in each case.

The President. Mr. Gill's photographs were obtained with exposures of 30 minutes, 40 minutes, and an hour.

Mr. De la Rue. But I got no trace of an image. I should be quite successful if I were to try now.

Mr. Christie. Though I have not attempted to make any drawing of the comet, Mr. Brett's drawing struck me as being so very different from the appearance that the comet presented to my eye, that I thought I would look at it again; and when I did look at it again, it appeared to me that there was a very faint portion of light on the northern side of the tail, so faint as to be hardly perceptible. Upon comparing the photographs with the drawing made by Mr. Brett, I find that while he has given a correct representation of the brightest part of the tail, as I saw it, he has not perceived this fainter portion. Therefore there seems to be a great discrepancy between the photographs and the drawings of some observers, which does not exist in fact, because they have not seen the fainter portion. It seems to be the same sort of difficulty which Mr. Stone had to encounter in regard to the solar eclipse of which he organized the observation in South Africa. Some persons gave an accurate representation of the brightest part only of the corona, while other observers drew its fainter extensions; and the two drawings seemed irreconcilable. That to me seems a solution of the difficulty. I put this forward with some diffidence, as I looked at the comet casually.

Mr. Brett. With what instrument?

Mr. Christie. I looked at it with the naked eye. With what instrument did you look at it?

Mr. Brett. With a binocular field-glass.

Mr. Christie. That would account for the difference.

The President. No doubt Mr. Brett has drawn accurately what he saw, and the Astronomer Royal has correctly described to us his observation; but the difficulty is to ensure that people really see the same thing. We see the importance of carefully considering this matter, lest we be inclined to infer very great changes from the absence or the appearance of faint beams of light in one man's drawing as compared with that of another.

Mr. Penrose. Would it not be interesting to the Society if we could ascertain from Mr. Gill or Mr. Ellery whether, before perihelion, they observed this duplicity of the nucleus?

The President. They might perhaps have obtained information if their attention had been directed to that matter in time.

Mr. Penrose. It is possible that they may have the information and have not thought it worth while to include it in their communications to the Society.

The President. We might make the inquiry.

Mr. Johnson. If the Secretary has received from Mr. Gill an account of a Meeting held in Cape Town, of which some friends in Liverpool have received a Report, there will be found some curious details which would no doubt be extremely interesting to the Society if they could be communicated. The Cape papers refer to five distinct patches of light separated by faint bands as having been seen in the nucleus.

Mr. Knobel. The communications received from Mr. Eddie, of Grahamstown, include one dated Sept. 13, which is prior to perihelion, when he examined the comet with a $9\frac{1}{2}$ -inch reflector. He there speaks of a large, extremely bright, well-defined nucleus, as bright as Venus and of a light golden hue. In his later communications he refers to the beaded character of the nucleus; and particularly mentions many nuclei as being detected on Oct. 21, when the comet's head resembled a sextuple star divided into two groups by a dark space.

Mr. Ranyard. With regard to the faint light extending from the nucleus towards the Sun, as described in the communication sent by Mr. Webb, it is also shown in Dr. Schmidt's beautiful drawing. But this tail is not necessarily due to matter from the comet streaming towards the Sun. Looking at the comet not very far from the plane of its orbit, and supposing it had a series of curved tails, we might see the end of one of these curved tails beyond the nucleus in the direction of the Sun. Many of the drawings may be thus explained. Mr. Brett's, for example; for he saw a great curved tail, described as being like the Greek letter *gamma*, very soft on one side, and very much harder on the other side, with a tuft in the middle, which is also shown in the beautiful photographs from the Cape. In Dr. Schmidt's drawing and in the communication sent by Mr. Webb's friend, the axis of the faint matter extending from the nucleus in the direction of the Sun is not parallel with the axis of the tail of the comet. These appearances would result if there were a series of fan-shaped tails stretching out in the plane of the orbit, in accordance with the theory of the matter being driven at different rates away from the Sun and dropping behind.

Mr. Knobel. There is nothing novel in a portion of the tail being projected towards the Sun. Encke's Comet in 1873 threw out a tail towards the Sun from the nucleus. There was an extension of an irregular character not bounded by any definite line that I could detect, the appearance being very similar to that described in this comet.

Capt. Noble said that he had observed the comet on November 9. The nucleus appeared elongated, like a nebula seen through a cylindrical lens. Its direction was inclined to that of the axis of the tail.

The President. With regard to the Transit of Venus, I am afraid most present have been very much disappointed—a greater disappointment in that it is quite hopeless for any of us ever to expect to see another transit. However, where the observers are scattered over a large portion of the earth's surface, we could only expect there would be failures in some places, as we might hope for successes in others. On the whole, we have very much to be thankful for. As far as the English expeditions are concerned, we have already received sufficient telegrams to assure us that our work has not been in vain. Undoubtedly a sufficient number of observations have been accumulated to give us a good deter-

mination of the Sun's distance, both from the ingress and from the egress observations. It may be known to most of the Fellows that in making the arrangements for the English Expedition we have chiefly relied on Delisle's method in preference to Halley's. The latter was undoubtedly the best at the time it was proposed; but it has ceased to be the best. The great advantage of Halley's method was that you were, to a great extent, freed from the absolute determination of the longitude; and in the last century longitudes could not be determined with very great accuracy. At present, with the extension of telegraphs and with steam-ships to convey our chronometers rapidly from one spot to another, longitude determinations present, comparatively speaking, little difficulty. This being the case, Delisle's undoubtedly becomes the best method, because it allows a larger selection of stations, and enables every observation made to be included in the equations of condition. The success of our method depends on our being able to obtain opposed contacts of accelerated and retarded phases. I am happy to say that we have received telegrams from Dr. Copeland, who is at Jamaica, showing that all the contacts have been observed there. (Applause.) Four contacts have been observed both by Dr. Copeland and by Capt. Mackinlay. With regard to Mr. Hall, nothing has yet been heard from him, probably because there is no telegraphic communication with his part of the island. We have also received a telegram from Mr. Talmage at Barbadoes, stating that Lieutenant Thomson and himself have secured the internal contact, an important one, at ingress, and the internal and external contacts at egress. That result is, so far, satisfactory. (Applause.) At Bermuda we have Mr. Plummer (in charge of station) and Lieut. Neate (in charge of longitude determinations). A 4½-inch telescope has been sent out from the Society and placed in the hands of Capt. Washington, R.E., who no doubt will do good service with it. We have not yet heard from that party, and it is not likely we shall for some days, because there is no telegraphic communication between Bermuda and New York; but we are not without hope that their expedition may have been successful. At all events we have got four good contacts at ingress, and probably eight contacts—seven we already know of—at the Cape. Therefore, at least from the ingress observations, we shall be able to arrive at a fair determination of the Sun's parallax. For the egress observations we have some observations at Barbadoes and Jamaica, and corresponding to them observations at New Zealand, the latter being in charge of Col. Tupman, who sends a telegram, from which it appears that he has not only observed the Transit, but is perfectly satisfied with the observations. The Transit has been observed also at Melbourne; so that we shall have four or six or even more corresponding observations to compare with one another at egress, from which we may also have a good determination of the Sun's distance. The observers at Brisbane have been unsuccessful; this is unfortunate, because their observations would have been of importance had cir-

cumstances favoured them. That is the weakest point in the whole chain. We have three excellent observers on the spot, and I greatly regret that they have not been fortunate. However in a large undertaking of this kind we do not expect to be successful at all points. I think enough has been said to reassure us that our work has not been entirely thrown away. On the other hand, we have heard that the Americans have been successful in their photographs at the Cape, also, I believe, in New Zealand, and to some extent in America. Although they have been partially interfered with by the clouds, we sincerely hope they have obtained for their method a thoroughly good trial. With regard to observations in Europe, they have been generally a failure. There have, however, been one or two exceptions. I am happy to find that Dr. Dreyer has been successful at Armagh. He says the Transit was well observed, and then he states that there were 35 seconds between the contact and the final separation of the limbs. Thirty-five seconds is a considerable quantity, and it appears to me most likely that he has observed what we used to call an "apparent contact." Now it is a curious coincidence that the time he gives—35 seconds—corresponds exactly, in angular measure, with the interval I observed between the apparent and the real contact at the Cape in 1874. My interval amounted to 53 seconds, but the motion of Venus was then so slow that it would correspond with 35 seconds in this transit. The coincidence is important as showing the reality of the conditions we have to deal with in these cases. Dr. Dreyer has given both the apparent contact and the final disappearance of the dark shade, and if all the other observers have done the same there may be a chance of success with both kinds of contact; but it is necessary to take the precaution of making a distinction between the two kinds of contact, for they take place with angular separations differing about a second and a half. I am in hopes that all our observers have carefully guarded against any such confusion. If they have, then, though I do not think we are ever going to pass at one step to a determination of the Sun's distance within 100,000 miles, yet I do believe we shall be able to fix the distance within something like 500,000 miles. If so, that will be a considerable step in advance of our present knowledge, and will undoubtedly answer the purpose of astronomers until it leads to some error sufficiently large to be put clearly in evidence, and which they will be able to remove by a yet more accurate determination of the Sun's distance. (Applause.)

Mr. Horner. May I ask if any intelligence has been received from Bath or from Bristol?

The President. I have merely seen the ordinary statement in the newspapers that there was a successful observation at Bath.

Mr. Horner. I asked the question because I was fortunate enough to see part of the Transit at Mells, ten miles south of Bath, and I shall be happy to place the results of my observations before the Society. (Hear, hear.) My brother saw the external contact with a spectroscope of two prisms.

Mr. Christie. I am sure we must all heartily join in congratulating the President on the success which has attended the observations, organized by him, of this Transit of Venus. (Applause.) I do not know whether the Fellows of the Society are aware how much time and trouble he has given to working out the scheme; but from my own knowledge and what I have seen I can bear testimony to the great and willing labour he has devoted to this enterprise. (Applause.) The scheme seems to have been very well planned for giving the determination of the Sun's distance both from ingress and egress. It is worthy of note that the English observations will do that by themselves, and the same may be said of the American observations, but not, I believe, of those of any other country. Although the observations at Brisbane have failed, the English enterprise may fairly be said to have been a success. (Applause.) With regard to Madagascar, I hope the observations there have been successful, and will form an important factor in the determination of the parallax from ingress. The general failure of the observations from the eastern portion of the British Islands is much to be regretted; but we must not altogether blame the Meteorological Office, because the forms they issued for the day of the Transit predicted what actually came to pass, namely, favourable weather in Ireland, in the north of Scotland, and in the west of England also. In the east of England, however, there was nothing but snow and bad weather predicted, and that prognostication was likewise fulfilled.

The President. I have had a notice put into my hands by *Mr. Lockyer* with reference to a matter of some importance, but it would, I feel sure, be more agreeable to the Society if he, instead of myself, would give some information about what he thinks it is desirable for us to attempt in reference to the next Eclipse of the Sun. (Hear, hear.)

Mr. Lockyer. I hold in my hand an American Ephemeris for next year, which shows that there is a very important eclipse in next May, observable from two islands in the South Pacific; and it may interest some of the Fellows to know that the Solar Physics Committee at their last meeting sent in an application to the Science and Art Department, pointing out how extremely important it was, from several points of view, that, if possible, observations of this eclipse should be secured—first of all because the duration of totality is exceedingly favourable, being $5\frac{1}{2}$ minutes; and secondly because, so far as we know at present, the eclipse will occur at the time of the maximum of the solar activity. I am the more anxious that the Royal Astronomical Society should be made acquainted with these facts, because I suppose that, in case the Government consider it desirable that observations should be secured (and, of course, in a matter of this kind it is absolutely impossible to secure observations without Government aid), it would be important that some members of this Society should gird up their loins and volunteer to take these observations. In

the first instance it was imagined that the only islands in the South Seas where observations could be made lay to the north of the Marquesas, and in those islands the duration was only 2½ minutes. But a more careful determination has shown that observations may be made to the west of the Marquesas, between these latter and the Friendly Islands, on the way to the Fijis, namely on Flint and Caroline Islands. We have been in communication with the French astronomers, and it is, I believe, quite settled that the French Government will send out one, if not two expeditions; and we are naturally quite anxious that we should have an English expedition if possible. (Applause.) With regard to the Americans, who are very much nearer to the seat of war, so to speak, than we are, I have had a telegram from *Prof. Young* informing me that the National Academy of Sciences is taking active measures in order to secure observations of this eclipse, if possible, and that they are in communication with the American Government; so that, in addition to one or two French parties, there will be an American party—*raison de plus* there should be an English one. If the English Government think that there should be observations taken, then possibly communications would be made to the Astronomical Society or the Royal Society asking for volunteers. It is upon that ground I bring this matter before you. I would suggest, in order to save time, if the expedition be organized, that any gentleman who may feel inclined to go the South Seas—"the sleepy hollow of the world"—whether to make observations regarding the solar atmosphere or the existence of intra-Mercurial planets, should at once send in his name to the Astronomer Royal. It is not possible to make arrangements long before the event comes off; and then, as soon as the Government agrees, if it does agree, to send an expedition, of course there will be an "ugly rush," and we shall have to work against time, which, so far as I know, is the most killing of all work.

Mr. Runyard. I am not sure whether there will be a Government expedition from America, but there is a likelihood of a private expedition, and I have been informed that those in charge of it will be happy to receive any English astronomers, if they are inclined to pay their share of the expenses. One Italian astronomer is going. The expedition will no doubt be a pleasant one; there is every chance of fine weather, and the Sun will have a good altitude during the five minutes of totality.

The President. If I understand Mr. Lockyer, he wishes those Fellows who would be willing and feel themselves competent to take part in the observations, to send in their names to the Astronomer Royal in order that we may know who would be ready to go as volunteers with this expedition.

Mr. Lockyer. It is very important that they should do so, that time may be saved.

The President. Dr. Dreyer, who has been appointed Astronomer to Armagh Observatory, has sent a circular stating that it is in-

tended, as a memorial to the late Dr. Robinson, to erect a new equatoreal there. A more suitable memorial to an astronomer than to improve the instrumental equipment of his own observatory could not be suggested. In accordance with Dr. Dreyer's request, I lay the circular on the table, in order that any Fellow may, if he should feel in a position to do so, contribute towards this worthy object.

Mr. Howlett. I have to submit to the Fellows of the Society a series of drawings of some of the principal solar spots, by which this present epoch of maximum is being remarkably characterized. The photographs now exhibited are from my original drawings, and these are of the great spot of April in the current year, and also that of the month of November last, the latter being the most recent as well as by far the largest outburst that has appeared on the disk for several years. This grand spot was readily visible by the naked eye. It seems to have first appeared on the disk on November 13; my first drawings were made on the 15th; and on the 19th, when it had obtained its greatest length and breadth. The dimensions of it given by Mr. Christie in 'Nature' seem perhaps excessive; but photographs of spots are apt sometimes to give the idea that the area occupied by them is larger than is the case. On the 19th it possessed an area of 3,462,000,000 square miles, which is about seventeen times the superficies of the whole terraqueous globe. These Sun-spots have been attended with the most striking displays of auroræ, the last one being rendered still more interesting by the unusual phenomenon of a rapidly drifting torpedo-shaped cloud of phosphorescent light which passed across the heavens from east to west in about two minutes. The correlation which exists between marked disturbances of the solar disk, auroral displays, and magnetic storms has been most strikingly confirmed. The maximum of the solar activity will probably extend well into 1883.

Mr. Howlett also mentioned that he had observed the Transit of Venus. The image of the Sun was projected on a screen. The first view of the planet was caught about five minutes past two, when it had advanced about 12 seconds on the disk. At the moment of the first internal contact the Sun was completely clouded. A good view of the planet was obtained when its following limb was about 12 seconds distant from that of the Sun's limb. At this time a dusky, tremulous, ill-defined ligament appeared to unite the planet with the Sun's following limb. Nothing like a bright spot was noticed on the planet. There were no solar spots of any importance on the Sun's disk with which to compare the comparative deepness of tints.

The President. It appears to have been very cloudy when Mr. Howlett saw this Transit of Venus, and in that case the atmospheric tremors would make the limb of the planet appear to jump backwards and forwards. What he describes as a ligament is, I think, due to that unsteadiness of the atmosphere. If there be

such a ligament I can only say it is much larger than any that anyone else has ever seen.

Mr. Howlett. I have a witness. My wife saw the ligament; but wives are sometimes prejudiced in one's favour. (Laughter.)

Mr. Christie. It is certainly to be hoped that the observations of the late Transit of Venus will be better than those of the last Transit of Mercury as made in Australia, because there was a large interval of time, more than a minute at any rate, between the several observations of contact. With regard to these interesting drawings of the Sun's spots, this year has been very remarkable, as giving four instances of coincidence between outbursts of Sun-spots and magnetic storms. I do not know whether such a thing has been remarked before to such a distinct degree. There were two outbursts of Sun-spots in April accompanied by two magnetic storms of a very remarkable character. There was an outburst of Sun-spots in October, accompanied by magnetic storms; then again a remarkable outburst in November, accompanied first by a very remarkable magnetic storm, and subsequently a sort of secondary storm with a great deal of disturbance of the magnets during the period immediately preceding. It will be very interesting to compare these drawings with the phases of the magnetic disturbance. With regard to the criticism of some of the dimensions of the spot which I had communicated to 'Nature,' they were intended rather for popular information than for scientific purposes. In giving the extreme length and the extreme breadth I never intended that the area of the spot should be calculated as if it were a rectangle. The area was given, in the proper scientific form, in millionths of the Sun's visible hemisphere. The extreme length and breadth were given in seconds of arc in order that people generally might be able to form some idea of the extraordinary dimensions of this spot, which was a most conspicuous object to the naked eye. In fact a number of soldiers who were going to the review on Saturday, November 18, stood on Blackheath looking at it as something wonderful. I received a letter from a farmer lad about this spot; he thought it was a Transit of Mercury which had not been calculated in the 'Nautical Almanac,' or of the hypothetical Vulcan.

Mr. Penrose. With regard to that auroral cloud, which, as Mr. Howlett said, passed from east to west, that phenomenon ought to have been seen in the north-west of France. It was seen at Rye and in the west of Devonshire, and seemed to pass across the entire sky in about two or three minutes. It must therefore have been visible over a very great extent of country. The observations which I read from different places seem hardly sufficient to fix the parallax; but possibly others may have seen it.

The President. From the description given me, it was something like the electrical clouds seen in the preceding aurora, not so much transference of absolute matter as of a particular state. From the descriptions we have seen, it must have been something more than any meteor absolutely moving through space.

Mr. Common. At the preceding auroral display I saw several of these clouds moving across the sky very slowly, and I could compare them to nothing more than to seeing a ray of light a little down its axis. I was forcibly reminded of a similar phenomenon which I had noticed last year. I had an electric light in my workshop, and through the skylight there was a beam of light across the fog. Looking at it I could see the beam above me, exactly like light of a magic lantern projected on a screen. It was really an optical image—a cross-section of the ray of light—and I was reminded of it when I saw the clouds in the southern part of the sky in the preceding auroral display.

The President. I examined the cloud with the spectroscope and saw that it gave a bright-line spectrum.

Capt. Noble. I would ask whether the correspondence between solar disturbances and magnetic storms is not as old as Sir E. Sabine. Mr. Hodgson and Mr. Carrington saw a solar outburst many years ago, which was absolutely coincident with a most remarkable magnetic storm.

Mr. Christie. Sir E. Sabine showed a *general* connection between the Sun's spots and the general magnetic disturbance throughout the year; but it is a very different matter to make out connection between any *particular* outburst of spots on the Sun and a magnetic storm. The particular case alluded to was a luminous outburst, not a Sun-spot; the account was received with some scepticism at the time, and I should hesitate to accept it as an unmistakable instance of connection. It was an isolated case, whereas this year attention has been called to four instances of connection between large spots on the Sun and magnetic storms. Hitherto I have always been doubtful as to the establishment of a direct connection between them. But here we have four distinct instances; and the newness of the thing arises simply from the very great disturbances now taking place on the Sun, and from its being rendered so plain to everybody that there is a connection between the particular spots and magnetic disturbances.

Mr. Ranyard. There is no doubt about the general coincidence between the cycles of magnetic disturbance and of Sun-spots; but the connection between individual instances is a different thing, and in the instances into which I have inquired I have found no such connection. Thus, in the case of the great prominence observed by Professor Young, there was no magnetic storm observed. There was no magnetic storm last year when the great prominence figured in the 'Comptes Rendus' was seen; nor was there any, I believe, in connection with the large spot photographed in 1870, at least there seemed to be none. So that there are occasionally large solar outbursts and no very definite correspondence to them in magnetic disturbance.

With regard to the invitation which Mr. Lockyer has given to the Society, such an invitation might be interpreted rather indiscriminately. I do not think that a great number of drawings and

general observations of eclipses are now needed, or that it would be worth while for the Government to send out an expedition to the other side of the world to make such observations. In 1871 a great deal of money was expended in sending out observers to India, who were instructed to make as many drawings of the corona as possible during totality, in order that they might see whether there was any change taking place in the corona during totality. Naturally there was a good deal of difference between the drawings; but we have now got past the era of drawings. What we want are photographs of the corona and specially contrived spectroscopic and polariscopic observations; and I do not think that it would be worth while to send out, at Government expense, observers who are not prepared for some such special observations.

Mr. Knobel. I observed the remarkable luminous cloud on the night of the aurora. I saw it when it was in the east some three degrees above the horizon. It was then perfectly parallel to the horizon, and appeared as a very bright streak, very narrow and much the brightest towards its centre. As it rose it got wider. As it passed across the meridian and disappeared in the west it became extremely broad and much less bright. It passed so rapidly, and I was so astonished at it, that I did not attempt to notice any thing which could indicate its parallax.

The President. I would make one further remark on the Transit of Venus with reference to the observation of Mr. Prince. Mr. Prince appears to have had a clear sky and to have made a careful and an accurate observation of the internal contact. The only regret is that, notwithstanding that in our instructions we particularly requested that the Dawes eyepiece should not be employed, yet Mr. Prince has made use of one. I regret that he did not follow the plan adopted by the other observers, as the actual value of an observation of this kind depends entirely upon its being comparable with others.

Capt. Noble. I saw a good deal of the Transit; but unfortunately did not get the contact, for it was actually snowing at the time and there were clouds in the way. I had sacrificed one beautiful tree in order to get the contact. (A laugh.) When I first got a glimpse of Venus it had been about five minutes on the Sun's disk. When the clouds cleared off I scrutinized the planet carefully, and noticed that there seemed to be no trace of an atmosphere surrounding Venus, the photosphere of the Sun being undimmed and unchanged right up to the limb of the planet.

The following papers were also announced:—

J. Tebbutt. "Observations of Comet Wells *a* 1882, made at Windsor, New South Wales."

N. de Konkoly. "Observations of the Great Comet *b* 1882, made at the Observatory, O'Gyalla, Hungary."

Prof. E. S. Holden. "The Star-gauges of Sir W. Herschel reduced to 1860."

Prof. J. C. Adams. "Observations of the Great Comet *b* 1882, made at the Cambridge Observatory."

Prof. J. C. Adams. "On Newton's Solution of Kepler's Problem."

Rev. J. Reed. "Observations of the Great Comet *b* 1882."

The following gentlemen were duly elected Fellows:—J. W. Appleton and the Hon. C. Duncombe.

The Meeting adjourned at 10^h 0^m.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Transit of Venus.

SIR,—

At Clevedon, the Transit was seen to perfection, from first contact until sunset, without one check or hindrance beyond the unsteadiness of the air. First contact was due at 2^h 0^m 30^s G.M.T., as computed from the 'Nautical Almanac' formulæ. I called time "49 seconds," which was noted by the assistant, but unhappily the minute was omitted. The clock was, however, known to be 2^m 41^s slow of G.M.T.; and, assuming the minute to have been 57, this works out to exactly the 2^h 0^m 30^s of the computation. Nevertheless I cannot be perfectly sure that the minute was not 58. It was plain, in any case, that with such unsteady definition the first contact could not be determined within many seconds. I will now give the substance of the notes taken down at the time from dictation as I sat at the instrument, a 3 $\frac{1}{4}$ -inch achromatic, using the full aperture with a power of 55 upon an erecting eyepiece with red screen, varied occasionally with one of neutral tint.

As soon as the Sun's limb was fairly broken into by the disk of the planet, I remarked that the cusps were not so sharply defined as in a solar eclipse, but that they appeared much brighter than other parts of the surface. This was very noticeable, more particularly on the southern cusp for a short time, when the planet was about one third on. The edge of the black disk was not so clearly cut as that of the Moon under similar circumstances, but it was far sharper at this time than at any later period of the transit, and it seemed to be margined with intense brightness. I take for granted this was an optical illusion caused by contrast; but it may as well find mention, since it ceased altogether when the planet had gained a less brilliant region of the photosphere. By the time the planet had gone a little more than halfway over the Sun's limb I began to be dimly conscious of the portion not yet entered, and when she was about two-thirds on I noted "Outer limb of Venus distinctly seen by refraction of light through her atmosphere." The effect was not that of projection of a dark body upon a light

background, but of picking out by a light bordering. Presently I was able to hold it steadily, and the note ran, "Luminous margin to dark limb very apparent." The breadth was quite appreciable; perhaps two or three seconds of arc. The illumination seemed tolerably uniform, except that perhaps the curve formed a trifle sooner next the Sun's limb. Certainly it could not this time be described as much brighter at one point than at another. The brightness went on increasing until the planet was fairly upon the disk.

The first internal contact was, to my surprise, even harder to determine accurately than the external. It is true the air was most particularly unsteady about that time. I could merely guess at the moment when the curve of the preceding margin ideally completed would be tangential with the Sun's limb, and I made it $2^h 26^m 3^s$, with the mortified feeling that this might be whole minutes too late. Such was probably the case, for it would make the interval between the contacts to be some five minutes longer than allowed by the formulæ. No doubt I was baffled by the "black ligament." I had been prepared for something pear-shaped, but here was a lengthening of the whole planet bodily. The effect was precisely that of a mirage throwing up the margin of an islet or headland when one is at the sea-level. Presently came the well-known under-cutting as of the mirage, and then for about ten seconds, the planet's outline being that of a very obtuse peg-top, a small black line or streak parallel with the two margins was rapidly waxing and waning as though striving to unite them. It hovered and fluttered as it were in the endeavour to pull itself into the shape of the Isle of Wight as shown upon a map, that it might bridge over the gap, or at any rate might just once more touch the border of the Sun, which it most seemed to desire, but all in vain. No connection as of the stalk of a pear was at any time fairly established, nor did any transverse black lines occur, nor any such light lines across the black streak. The fluctuations were unmistakably connected with the unsteadiness of our atmosphere, being rhythmical with the air-ripples playing along the Sun's limb.

The planet upon the solar disk was never for a moment perfectly still, but was working in an ever-varying polygon, as when an orange is kneaded in the hands. It was evident, however, that the definition of the margin remained throughout less sharp than that of the Moon would be. No opinion could be formed as to any spheroidal flattening. By an optical illusion a patch of greyish light was usually seen upon the planet's disk, open in the centre. It suggested a ring nebula in its general aspect, its extreme diameter being about two thirds of that of the planet. My impression is that this patch was only seen during the early part of the transit, ceasing to show as the Sun got lower. There was a group of small spots about halfway across the Sun, dotted upon a very extensive penumbra. Contrasted with the black disk, these looked merely brown, and by no means a deep brown. I tried a higher

power, in the hope of seeing a nucleus-dot for comparing, but the air would not bear it. At no time was there the remotest trace of any thing that could suggest the fabled satellite.

As the Sun approached the horizon, the planet was conspicuous to the naked eye, even without the intervention of a screen. A small band of cloud was crossed, which served to render the black dot all the more manifest by hiding the upper portion of the Sun as the lower emerged. Then for some two minutes the entire spectacle was displayed for the last time; the Sun and the planet sank gently together into the hills in the remote distance; and no sooner were they gone than the clouds gathered, the sky became overcast, and within a few hours the whole country was buried in snow.

East Clevedon Vicarage, Somerset.
1882, Dec. 7.

STEPHEN H. SAXBY.

P.S.—Since writing the above, I have been collecting testimony from various parts of our west country as to the conditions of the transit. The nearest place I can hear of at which the contacts were seen absolutely free from any distortion is Silverton, near Exeter, some 50 miles further from the edge of the approaching storm, where Mr. Langdon enjoyed a perfectly steady air, and saw no symptom of the black ligament. This famous phenomenon seems after all to be nothing more than an effect of the undulation of the local atmosphere, wholly independent alike of instrument and of observer.

S. H. S.

SIR,—

We had made ready to observe the two first contacts, external and internal, with the greatest care, and to make one or other of the observations recommended in the instructions which have been published for this occurrence by the Washington Observatory. I would here say a word on some of the most interesting points observed.

Before the phenomenon commenced the western part of the sky, where the Sun was, was laden by stratiform clouds, which, sometimes becoming thinner, sometimes denser, made the limb of the Sun very tremulous, and consequently rendered the observation uncertain; and this part of the sky remained in this condition till sunset.

On this account we were obliged to give up the spectroscopic observations which we had intended to make. On the other hand, we occupied ourselves in determining the instants of the two contacts with the greatest attention possible. I observed with the Merz refractor of 4 inches aperture. The magnifying-power employed was 54 for the external contact, and 120 for the internal contact. The following are the results we obtained:—

December 6, 1882.

First external contact, $2^h 49^m 31^{\cdot}0$ Rome Mean Time.:

First internal contact, 3 9 54'4 " "

The instant of internal contact had already passed when the dark disk of Venus was detached from the limb of the Sun, but Venus was still distinctly seen to be connected with the latter by means of the *black drop*.

I was careful to notice if, after the external contact, any aureola of light could be distinguished round the disk of the planet, evidence of the atmosphere of Venus lighted up by the Sun; but I could not discover it, not even on that part of the limb of the planet which was furthest from the Sun, the air being always laden with moisture and somewhat tremulous.

The disk of Venus did not seem to be entirely black; it had a tint between a light red and a dark yellow, and its shape kept altering on account of the great quantity of vapour present in the air, the influence of which increased continually as the Sun approached the horizon. On this account the determinations we made of the diameter of the planet are not very certain.

The Observatory of Moncalieri,
1882, Dec. 20.

P. F. DENZA,
Director of the Observatory.

Effect of Irradiation with Differing Apertures.

SIR,—

Some few months ago it occurred to me that it might be interesting as an experiment, if nothing more, to attempt to determine the effect of irradiation in enlarging the Moon's disk, by measuring the distance from the limb of a well-defined point at a convenient distance from it, using widely differing telescopic apertures. Accordingly, on May 27, I took, with a wire-micrometer, three measures of the distance of a bright point from the limb, first with the full aperture of the telescope, $7\frac{1}{8}$ inches, and then with the aperture reduced to 2 inches, with results as given below:—

$7\frac{1}{8}$ -inch aperture.	2-inch aperture.
54''·52	56''·71
54·88	56·71
54·88	56·35

The respective means being 54''·76 and 56''·59, and the difference between them 1''·83, which is rather smaller than I expected.

I had imagined that the difference would probably follow the law of the first bright diffraction ring of a stellar image. Now the radius of the first bright ring for a $7\frac{1}{8}$ -inch aperture is 1''·01, and for one of 2 inches is 3''·70, the difference being 2''·69, or nearly 1'' greater than results from my measures. At the same time it must be admitted that the measures are too few to be relied on for giving an exact result.

Whether it would serve any useful purpose to push the inquiry further is perhaps doubtful. It is possible, however, that these preliminary measures may possess sufficient passing interest to warrant their being brought to the notice of the readers of the 'Observatory.'

I am, Sir, yours faithfully,

GEORGE KNOTT.

Knowles Lodge, Cuckfield.
1882, Dec. 13.

The relative Heights of some of the Jovian Markings.

SIR,—

It seems to have been long generally accepted by astronomers that the dark belts and other markings visible on Jupiter are situate at a lower level than the bright surface, and even that they were portions of the solid body of the planet. In the 'Observatory' for March 1882, p. 84, I gave some reasons for believing that, contrary to this opinion, the two principal equatoreal bands are really *above* the general apparent surface. During the last few months some attention has been directed here to the different relative heights of the various markings, which seem to support the following conclusions.

South of the equatoreal region, the objects which are deepest down appear to be the small white spots, which have approximately the same rotation-period as the red spot. Above these comes the red spot, which certainly seems to be higher than the former spots, though perhaps not to any great extent. Higher still float those dusky ill-defined clouds which have a period of about $9^h 55^m 16^s$, and are sometimes as large as the red spot. It is just possible even that the present abnormal faintness of this object may be caused by one of these dusky masses being suspended over it.

In the equatoreal region evidence is much more conclusive. First, or lowest, are the numerous white spots and patches between the two principal dark belts. Occasionally some of these objects were seen extending north of the northern dark belt, which last remained unaltered, with the exception of being more sharply defined. The southern dark belt is much more decided. Any one who gives more than a casual glance at the planet cannot fail to have noticed how the white patches, and particularly the small white spots just on the northern edge, are constantly being obscured by outlying vapours of the belt. At the present time the south equatoreal dark belt forms, with the similar band immediately south, a beautiful deep red double belt, extending nearly round the planet. I have several times lately seen a brilliant white spot on the north edge of the northern component of this double belt extending also south of it, so as to be visible just beyond the southern edge, and even to fill the whole interior space of the double belt. Under these circumstances the south equatoreal dark band (which is the northern component) has appeared like a dark bar, apparently somewhat narrower than the rest of the belt, stretching across the spot and dividing it into two. The belt here is perfectly distinct and well defined, its greater narrowness being probably caused by deception arising from the intense brilliancy of the white spot, or perhaps the edges of the belt might be less dense and allow the spot to be seen through.

The two principal dark equatoreal bands, or their upper surfaces at least, are thus evidently situate above the white spots, and higher still are the dark spots which appear on these zones. The fact that the dusky belts might be the real body of the planet "is sup-

posed to be established by the invisibility of the darkest spots when near the limbs, and the fading away of the grey streaks towards their ends"; but I have frequently seen the dark spots on the southern belt right up to the limb with great distinctness, and these objects are more conspicuous when near this position than when they are on the centre—a circumstance which, though easily accounted for on the supposition that they are above the general level of the belt, adds considerably to the difficulty of obtaining accurate observations of their passage over the central meridian. In fact I have frequently seen some of these spots distinctly when near the following limb, although when central they were too faint to be observed, or even seen at all.

In the northern hemisphere the third belt from the north and the spots upon it evidently bear much the same resemblance as the equatorial dark bands to the surrounding surface.

The possibility of the present faintness of the red spot being caused by one of the faint dusky masses floating over it is referred to above. One of these patches was seen in 1880 attached to the preceding end of the red spot, and gradually to increase its distance. The short period of time over which the observations extended, and the faintness of the object, unfortunately prevent any very accurate result from being obtained; but this patch, supposing it to be in existence, would now be a few minutes past the centre of the red spot, having completed one revolution with respect thereto. If this should turn out to be correct, we may shortly expect to see the red spot recovering some, at least, of its former glory.

Yours faithfully,

West Brighton, 1882, Dec. 2.

A. STANLEY WILLIAMS.

The August Cepheids.

SIR,—

The shower of bright meteors occurring on August 9–11, from the point $335^{\circ} + 73^{\circ}$ in Cepheus, to which I drew attention in the 'Monthly Notices' for November 1882, p. 37–8, has, it appears, been conspicuously visible during the present year. Mr. E. F. Sawyer of Cambridgeport, Mass., in describing his recent observations of the Perseids ('Science Observer,' Nos. 39–40, p. 31), says:—"The radiant-point of the Perseids appeared to lie at its normal position near α Persei. The majority of the unconformable meteors radiated from a position near β Cephei, $320^{\circ} + 72^{\circ}$."

Mr. Sawyer's observations were made chiefly on August 9, when he noted several of these brilliant Cepheids. At $11^{\text{h}} 1^{\text{m}}$ a meteor, exceeding the first magnitude, pursued a rapid course from $12^{\circ} + 69^{\circ}$ to $32\frac{1}{2}^{\circ} + 60^{\circ}$ (near ϵ Cassiopeiæ), and at $12^{\text{h}} 35^{\text{m}}$ another, scarcely less brilliant, was seen near α Draconis, with a path from $220^{\circ} + 74^{\circ}$ to $197\frac{1}{2}^{\circ} + 63^{\circ}$. It is satisfactory to note Mr. Sawyer's excellent confirmation of this important meteor-shower.

I am, Sir, yours faithfully,

Bristol, 1882, Dec. 18.

W. F. DENNING.

* 'Celestial Objects,' 4to ed. p. 158.

The November Leonids, 1882.

SIR,—

The watch for meteors belonging to this well-known meteor-stream on the nights of the 11–15 of November was interrupted by a cloudy sky on the 12th, but was unimpeded on each of the other nights. The following table exhibits in a condensed form the very meagre results obtained, and indicates that the Earth passed through a very attenuated part of the cometary meteor-stream :—

1882. Nov.	Duration of Watch.	Length of Watch.	Number seen.		Remarks.
			All Meteors.	Leonids.	
	From to h m h m	m			
11.	16 00 16 15	15	0	0	Clear.
12.	Cloudy.				Cloudy.
13.	16 00 16 30	30	4	2	Clear. } Radiant-point
14.	16 55 17 30	35	5	3	Very clear. } R.A. 148° + 23°.
15.	13 30 14 00	30	1	1	Meteors very scarce.
11–15.		1 ^h 50 ^m	10	6	

Limit of view restricted to $\frac{1}{8}$ of the visible heavens. Centre of observation, Leo.

Yours &c.,

Cambridgeport, 1882, Nov. 19.

EDWIN F. SAWYER.

Sir Robert Moray and Saturn's Ring.

SIR,—

My attention has been called to a note by Mr. H. Rix, from which it appears that the archives of the Royal Society show that the "Person" to whom William Ball (or Balle) sent his drawing and note on Saturn's Ring in 1665 was neither Wallis nor Hooker, but Sir Robert Moray, one of the principal founders of the Royal Society, and who acted as its temporary President until its incorporation in 1662 (when Lord Brouncker was elected the first regular President). Sir Robert was partly educated in France, and served in the French army under Louis XIII., but came to England during the civil war and entered into the service of Charles I., whose escape he endeavoured to effect in 1646. After the Restoration, he became a Privy Councillor for Scotland. In science he seems to have given most attention to chemistry, but published also papers on the tides, and on experiments for improving the art of gunnery. It does not appear that he ever practised observations in astronomy; this may perhaps help to account for his conjecture that the appearance which Saturn's ring seemed to present in Ball's drawing proceeded from its consisting of two bodies instead of one, which expression has unfortunately led so many to fancy that Ball anticipated Cassini in the discovery of the

duplicity of the ring. Kitchener was partly led to think so from believing that the Balls ought to have seen it with a telescope of the size they used, in which we cannot agree with him.

Yours faithfully,

Blackheath, 1882, Dec. 19.

W. T. LYNN.

NOTES.

PROF. CHALLIS.—We regret to record the death of the Rev. James Challis, Plumian Professor of Astronomy in the University of Cambridge, on Dec. 3, in the 79th year of his age. Prof. Challis was born at Braintree, in Essex, and after proceeding to Trinity College, Cambridge, graduated as Senior Wrangler and First Smith's prizeman in 1825, becoming a Fellow of his College in the following year. In 1836 he was appointed Plumian Professor and Director of the Cambridge Observatory, as successor to Prof. Airy; and during the twenty-five years that he had charge of the Observatory he accumulated with great industry a great store of observations of the Sun, stars, small planets, and comets, the results of which are published in the series of volumes of Cambridge Observations and in various scientific transactions. Though his failing health compelled him to resign the charge of the Observatory in 1861, he continued to lecture on practical astronomy to a select class of students; and in 1879 he published his 'Lectures on Practical Astronomy,' a valuable work in which the Cambridge instruments and the methods of observing with them are specially discussed, with less detailed reference to other instruments. Among the matters to which Prof. Challis has devoted special attention may be mentioned the determination of flexure for oblique positions of the telescope of the mural circle and the error of form of the transit pivots. Throughout his life Prof. Challis gave much attention to the physical side of hydrodynamics. In 1833 he read to the British Association a Report on the analytical theory of Hydrostatics and Hydrodynamics, and he subsequently evolved his Hydrodynamical theory of the Physical Forces by which he sought to explain physical phenomena. This theory was elaborated in his 'Principles of Mathematics and Physics,' published in 1869, and in an essay on the Mathematical principles of Physics, published in 1873.

W. H. M. C.

PROF. HENRY DRAPER, M.D., whose premature death at the age of 45, we have to regret, was a son of the late Prof. J. W. Draper, of New York University, where he was educated, graduating as M.D. in 1858, and being elected to a Professorship in the University two years later. He early devoted himself to astronomy, commencing by the construction of a silvered glass reflecting telescope of $15\frac{1}{2}$ inches aperture, with which he took some fine photographs of the Moon. He subsequently made an equatoreal

reflector of 28 inches aperture, with which he carried on for ten years important researches in celestial photography. Among his achievements were photographs of stars and star-clusters, of the spectra of stars, and, quite recently, of the nebula in Orion and its spectrum. Though he was anticipated by Dr. Huggins in the application of photography to the spectra of stars and of the nebula in Orion, Dr. H. Draper's work was quite independent, and, as regards the Orion nebula, he made the important discovery that there are two condensed portions which give a continuous spectrum. Dr. H. Draper has also done good work in solar spectroscopy. In 1872 he obtained an excellent photograph of the diffraction spectrum from below G to O in the ultra-violet, and subsequently he made a number of comparisons of the photographic spectrum of oxygen with the solar spectrum to show that certain spaces or bright lines in the latter indicated the presence of this element in the Sun. Among his services to science must also be mentioned the superintendence of the American photographic operations in the Transit of Venus 1874, which has resulted so successfully, and in commemoration of which the United States government caused a special gold medal to be struck and presented to him.

W. H. M. C.

THE TRANSIT OF VENUS.—A most gratifying success seems, on the whole, to have attended the various expeditions sent out from different countries to observe the recent transit. The British expeditions have been all but uniformly successful; and the observations obtained by them will be sufficient, independently of all observations made by astronomers of other nations, to furnish a good determination of the parallax, alike from the contacts at ingress and egress. In the West Indies, where the whole transit was visible, the ingress being retarded and egress accelerated, all four contacts were observed by Dr. Copeland in Jamaica, and all but the external contact at ingress were obtained at the Barbadoes station. At Bermuda, also, all four contacts were secured, those at ingress under highly favourable conditions; the egress had, however, to be watched amid clouds. The South-African observers (ingress accelerated) were all favoured with good weather, from D'Urban, where Mr. Neison had gone, round to Cape Town; and the news from Madagascar, where Father Perry was stationed, is also satisfactory. The third or Australian group of stations (egress retarded) has not been able to record quite such an unqualified success; still even here enough has been achieved to amply secure the end in view. For much of the east coast of Australia the transit was a failure. It was not seen at Brisbane or at Sydney, rain falling over a great portion of Queensland, Gippsland, and New South Wales. But, as a set off to this disappointment, Col. Tupman has been able to telegraph a complete success from New Zealand; and the transit was observed at Melbourne, where 23 photographs were taken, at Adelaide, at Hobart Town, and at one or two places in New South Wales.

Astronomers at home had prepared for the transit with many fears; the chances of good observing weather in December and with the Sun so low down in the west seemed small indeed. The actual event therefore could scarcely be called a disappointment, it so closely corresponded to what might have reasonably been expected. A dense veil of cloud concealed the Sun from view at all the principal English observatories; at Greenwich, Oxford, and Cambridge nothing could be done. But the Irish observatories were more favoured. Dr. Ball, at Dunsink, was not indeed able to secure the contacts, but had a clear Sun from $2^h 37^m$ to $3^h 3^m$, and obtained a number of measures of the distances between the limbs; whilst Dr. Dreyer, at Armagh, and Dr. Doberck, at Markree, saw the contacts under fairly good conditions. Observers in Devon, Cornwall, and S. Wales also enjoyed clear skies during the transit; and in Scotland the gallant observer of Ben Nevis, Mr. Wragge, watched the phenomenon throughout.

Observers in the rest of Europe were scarcely so fortunate even as the average of British astronomers. The principal French observatories shared the same fate as Greenwich and Oxford; an expedition by the Brothers Henry to the summit of the Pic du Midi, resulted only in failure. At Marseilles both the external and internal contacts were observed, but through cloud, an experience which also befell M. Lescarbault at Chateaudun. At Cannes the transit was well seen. MM. Thollon and Gouy, who had gone to Portugal to observe the phenomenon, met with complete disappointment. Still their fate was hardly so tantalizing as that of the observers in Southern Spain, where Sig. Arcimis records that there had been persistent fine weather, without a drop of rain, from May until December 6, on which day rain fell heavily till a little after sunset, when the sky cleared again. The German astronomers were more favoured, and a number of photographs were taken at Potsdam. Prof. Tacchini secured the contact at the Collegio Romano, and made some most interesting spectroscopic and physical observations, though the weather was somewhat cloudy. M. Janssen at Oran also obtained some valuable spectroscopic results and several capital photographs 30 centimetres in diameter.

In America a heavy snowstorm defeated the astronomers in Canada generally, and over much of the northern portion of the United States; at St. Louis and Cincinnati nothing could be done. But a very large number of successful observations must still have been secured. All four contacts were observed at the Naval Observatory, Washington, at Haverford College, Philadelphia, at Yale College, at Harvard and the Johns Hopkins Universities, and doubtless at very many other places from which information has not reached us. Dr. Müller's expedition at Hartford, Connecticut, secured 180 photographs, 8 complete sets of heliometer measures, and all the contacts but the first. In the southern portion of the States, the success of all parties was most gratifying. The French Expedition to Florida under Col. Perrier obtained 200

photographs. The United States mission at Cedar Keys obtained continuous photographs, though the external contact at ingress was lost. Prof. Asaph Hall in Texas secured 204 photographs. The first two contacts were, however, lost here and by the German expedition to South Carolina. Nevertheless three complete sets of heliometer measures were made at the latter station. At San Francisco 48 photographs were taken; and M. Bouquet de la Grye at Puebla, Mexico, and M. d'Abbadie at Santiago de Cuba report unqualified successes, successes the more important that M. Tisserand at Martinique was only able to observe the first external contact.

Of expeditions in the southern hemisphere, Prof. Newcomb's party in Cape Colony took 236 photographs; and the two parties of the Argentine Republic, the one under M. Beuf at Buenos Ayres, the other in the south of the province under M. Perrin, and the French expeditions to Buenos Ayres and to Santiago de Chile, all met with complete success. It will thus be seen that, on the whole, the failures have been but few, and even where the meteorological conditions have been generally adverse in any district, there have still been places in or near it where the transit was well observed. The general result, therefore, may fairly be described as most satisfactory, and there is every prospect that we shall have as accurate a determination of the solar parallax, both from the observations of contacts and from the photographs, as the method of transits can be expected to supply; whilst the spectroscopic observations made by MM. Janssen, Tacchini, and others promise interesting information as to the physical condition of the planet Venus. It may be added that a careful scrutiny showed no trace of any satellite.

E. W. M.

A METHOD OF PHOTOGRAPHING THE CORONA.—At the meeting of the Royal Society on Dec. 21, Dr. Huggins announced that he had succeeded in obtaining photographs of the corona without the intervention of an eclipse. An examination of the photographs of the coronal spectrum taken in Egypt during the total eclipse of the Sun had shown Dr. Huggins that the maximum intensity of the coronal light was in the violet between G and H, and he was thus led to try whether by the use of violet glass, which would absorb all but this portion of the spectrum, he could not make the corona stand out against the glare of scattered sunlight enfeebled by the absorptive medium. Applying this principle he obtained with a Newtonian reflector a number of photographs showing the characteristic rays and structure of the corona round the Sun. In some of these the Sun, and in others both the Sun and corona were reversed by overexposure, so that they gave a positive image, whilst others gave a negative image, *i. e.* black for white; but in all of them the coronal structure was to be seen more or less readily, and a comparison with a photograph of the corona taken during the late eclipse showed a remarkable agreement in the characteristic rays. Prof. Stokes and Capt. Abney, who had carefully

examined Dr. Huggins's photographs, were convinced that what he had photographed was really the corona and not mere glare round the Sun. Dr. Huggins hopes that the application of his method at some elevated station where the sky is purer may enable us to obtain daily photographs of the corona, leading to a great increase in our knowledge of the Sun and his surroundings. W. H. M. C.

CERASKI'S VARIABLE, T CEPHEI*.—Mr. Knott has observed the following maximum and minima of this star (D.M. +67°, No. 1291):—

Min. 1881 June 18, Mag. 9.5.

Max. 1882 Jan. 13, Mag. 6.4.

Min. 1882 July 23, Mag. 9.8.

The interval between the two minima is 400 days. The next maximum and minimum may therefore be expected to fall about 1883, February 17 and August 27. The star's place for 1880.0 is given by Herr Ceraski as $\alpha = 21^h 7^m 57^s.05$, $\delta = +68^\circ 0' 8''.4$.

We learn that M. E. L. Trouvelot has left the United States, and has joined M. Janssen at the Observatory of Physical Astronomy, Meudon, near Paris.

Ephemeris of the Great Comet, b 1882 †.

By Dr. H. OPPENHEIM.

Computed from the elements of Mr. S. C. Chandler, reduced to Mean Equinox 1883.0.

For Berlin Midnight.

	B.A.	Decl.	Log. r.	Log. Δ.
1883.	h m s			
Jan. 1....	7 12 27	−29 18.5	0.3910	0.2394
3....	7 6 52	28 58.6		
5....	7 1 28	28 36.8	0.4017	0.2519
7....	6 56 17	28 13.2		
9....	6 51 18	27 48.0	0.4120	0.2654
11....	6 46 32	27 21.3		
13....	6 42 0	26 53.3	0.4220	0.2798
15....	6 37 41	26 24.3		
17....	6 33 36	25 54.3	0.4317	0.2949
19....	6 29 44	25 23.6		
21....	6 26 6	24 52.4	0.4410	0.3104
23....	6 22 41	24 20.7		
25....	6 19 29	23 48.7	0.4501	0.3264
27....	6 16 29	23 16.5		
29....	6 13 42	22 44.1	0.4589	0.3427
31....	6 11 7	22 11.7		
Feb. 2....	6 8 44	21 39.5	0.4674	0.3591

* Astron. Nachr. No. 2469.

† Dun-Echt Circular, No. 67.

Approximate Times of Transit of the great Red Spot and Equatorial White Spot across the Central Meridian of Jupiter in January 1883.

Date.	Red spot on C.M.	White spot on C.M.	Date.	Red spot on C.M.	White spot on C.M.
	h	h		h	h
1.	12 ⁹	9 ²	17.	6 ¹	8 ⁷
2.	8 ⁸	14 ⁷	18.	11 ⁹	14 ³
3.	14 ⁵	10 ⁴	19.	7 ⁸	9 ⁹
4.	10 ⁴	6 ¹	20.	13 ⁵	5 ⁶
5.	6 ³	11 ⁶	21.	9 ⁴	11 ¹
6.	12 ⁰	7 ³	22.	5 ³	6 ⁸
7.	7 ⁹	12 ⁷	23.	11 ⁰	12 ³
8.	13 ⁶	8 ⁴	24.	6 ⁹	8 ⁰
9.	9 ⁵	13 ⁹	25.	12 ⁶	13 ⁴
10.	5 ⁴	9 ⁶	26.	8 ⁵	9 ¹
11.	11 ¹	5 ³	27.	4 ³	4 ⁸
12.	7 ⁰	10 ⁷	28.	10 ¹	10 ³
13.	12 ⁷	6 ⁴	29.	6 ⁰	6 ⁰
14.	8 ⁶	11 ⁹	30.	11 ⁸	11 ⁵
15.	4 ⁴	7 ⁶	31.	7 ⁷	7 ²
16.	10 ²	13 ¹	Feb. 1.	13 ⁴	12 ⁶

The following are a few of the recent observations of these markings obtained here, subsequently to those published in the list for December :—

1882.	Red spot observed on C.M.	1882.	White spot observed on C.M.
Nov. 27....	19 ^h 10 ^m	Nov. 26....	7 ^h 45 ^m
Dec. 4....	19 52	26....	17 37
8....	13 7	28....	18 46
9....	9 1	29....	14 29
15....	13 55	30....	10 10

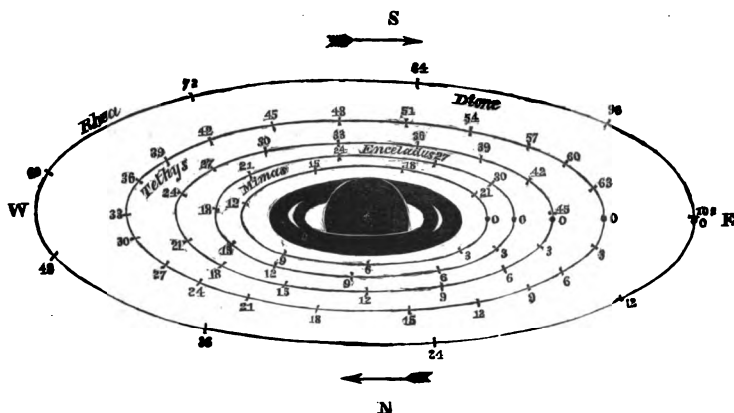
Now that Jupiter is visible all night, it frequently happens that two transits of the markings may be observed on the same date. Thus, on Jan. 5 the red spot will be central at about 6^h.3 and again at 16^h.2. On Jan. 4 the white spot will transit at about 6^h.1, and again at 15^h.9.

The conjunction of the two spots will probably occur on Jan. 29.

W. F. DENNING.

Bristol, 1882, Dec. 20.

*Apparent Orbits of the Five Inner Satellites of Saturn as
seen in an Inverting Telescope.*



The arrows in the diagram show the direction of the motion of the satellites. The figures indicate the interval in hours from the time of last East elongation. The semi-major axis of the orbit of Rhea is $85''\cdot7$ on Jan. 1, and $81''\cdot4$ on Jan. 31.

The following table gives the Greenwich Mean Time of the East elongations of the satellites. In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS (East).		ENCELADUS.		TETHYS.		DIONE.	
h		h		h		h	
Jan.	1 12'3	Jan.	1 19'4	Jan.	2 16'6	Jan.	1 7'7
	2 10'9		3 4'3		4 13'9		4 1'4
	3 9'5		4 13'2		6 11'2		6 19'1
	4 8'1		5 22'0		8 8'5		9 12'7
	5 6'7		7 6'9		10 5'8		12 6'4
	6 5'3		8 15'8		12 3'1		15 0'1
	17 12'7		10 0'7		14 0'4		17 17'8
	18 11'4		11 9'6		15 21'7		20 11'4
	19 10'0		12 18'5		17 19'1		23 5'2
			14 3'4		19 16'4		25 22'8
			15 12'3		21 13'7		28 16'5
			16 21'2		23 11'0		31 10'2
			18 6'1		25 8'3		
			19 14'9		27 5'7		
			20 23'8		29 3'0		
			22 8'7		31 0'2		
			23 17'6				
			25 2'5				
			26 11'4				
			27 20'3				
			29 5'1				
			30 14'0				
			31 22'9				

MIMAS (West).		RHEA.	
h		h	
Jan.	8 13'9	Jan.	2 11'2
	9 12'5		6 23'5
	10 11'1		11 12'0
	11 9'7		16 0'4
	12 8'4		20 12'8
	13 7'0		25 1'3
	14 5'6		29 13'7
	27 10'2		
	28 8'8		
	29 7'4		
	30 6'0		

Astronomical Memoranda, 1883, January.

Sun. Jan. 1, sets $4^h 0^m$, rises $20^h 8^m$; Jan. 11, sets $4^h 12^m$, rises $20^h 4^m$; Jan. 21, sets $4^h 28^m$, rises $19^h 54^m$; Jan. 31, sets $4^h 46^m$, rises $19^h 41^m$.

Equation of Time:—Sun after Clock, Jan. 1, $3^m 45^s$; Jan. 11, $8^m 8^s$; Jan. 21, $11^m 32^s$; Jan. 31, $13^m 40^s$.

Sidereal Time at Mean Noon:—Jan. 1, $18^h 43^m.2$; Jan. 11, $19^h 22^m.6$; Jan. 21, $20^h 2^m.0$; Jan. 31, $20^h 41^m.5$.

<i>Moon. rises.</i>		<i>sets.</i>		<i>sets.</i>	
	<i>h m</i>		<i>h m</i>		<i>h m</i>
Jan. 1.	13 2	Jan. 11.	7 34	Jan. 21.	18 48
2.	14 7	12.	8 53	22.	<i>rises.</i>
3.	15 10	13.	10 14	23.	5 21
4.	16 15	14.	11 31	24.	6 27
5.	17 15	15.	12 48	25.	7 34
6.	18 12	16.	14 4	26.	8 38
7.	19 1	17.	15 16	27.	9 43
8.	<i>sets.</i>	18.	16 22	28.	10 47
9.	4 59	19.	17 20	29.	11 51
10.	6 15	20.	18 7	30.	12 55
				31.	13 58

Last Quarter, Jan. 1, $0^h 50^m$; New Moon, Jan. 8, $17^h 59^m$; First Quarter, Jan. 15, $12^h 48^m$; Full Moon, Jan. 22, $19^h 16^m$; Last Quarter, Jan. 30, $22^h 27^m$.

Mercury is an evening star, at his greatest elongation, $18^\circ 32'$ E., Jan. 21, 23^h , when he is favourably placed for observation. He is stationary Jan. 27, 0^h .

Venus is a morning star, in perihelion Jan. 9, 6^h , and at her greatest brilliancy, Jan. 10, 15^h . Diameter:—Jan. 1, $46''.8$; Jan. 31, $29''.4$. Illuminated portion of disk, 0.293 on Jan. 15.

Jan. 1, R.A. $16^h 25^m.7$, Dec. $17^\circ 5'$ S., tr. $21^h 40^m$, rises $17^h 11^m$
31, 17 41 3, 19 0 S., 21 0 16 43

Jupiter is retrograding in Taurus. Diameter:—Jan. 1, $44''.2$; Jan. 31, $41''.4$.

Jan. 1, R.A. $5^h 36^m.0$, Dec. $23^\circ 1'$ N., tr. $10^h 51^m$, sets $19^h 0^m$
31, 5 24 0, 22 57 N., 8 41 16 49

Saturn is in Taurus, stationary Jan. 20, 20^h .

Jan. 1, R.A. $3^h 11^m.2$, Dec. $15^\circ 27'$ N., tr. $8^h 27^m$, sets $15^h 48^m$
31, 3 10 1, 15 32 N., 6 28 13 49

<i>Outer Ring.</i>		<i>Inner Ring.</i>		<i>Ball.</i>
<i>Maj. Axis.</i>	<i>Min. Axis.</i>	<i>Maj. Axis.</i>	<i>Min. Axis.</i>	<i>Diam.</i>
1882, Dec. 30..	$44'' 38$	$16'' 87$	$29'' 51$	$11'' 22$
1883, Jan. 19..	$42'' 93$	$16'' 32$	$28'' 55$	$10'' 85$
				17 1

The south side of the rings is visible, the elevation of the Earth above their plane being $22^\circ 21'$ S. on 1882, Dec. 30, and $22^\circ 20'$ S. on 1883, Jan. 19, and of the Sun $23^\circ 34'$ S. and $23^\circ 44'$ S.

Uranus may be found about 1° *n* and 8^m *p* of β Virginis.

Jan. 1, R.A. $11^h 36^m.8$, Dec. $3^{\circ} 21' N.$, tr. $16^h 51^m$
 31, 11 35 '0, 3 34 N., 14 51

Neptune is stationary in Aries, Jan. 26, 9^h .

Jan. 1, R.A. $2^h 57^m.1$, Dec. $14^{\circ} 58' N.$, tr. $8^h 12^m$
 31, 2 56 '4, 14 57 N., 6 14

Phenomena.

G. M. T.		
h m		
Jan. 1	8 46	J. ii. Oc. D.
	12 12	J. ii. Ec. R.
	14 39 49	Virginis
		Oc. R. 259° .
	16 11	J. i. Tr. I.
2	13 18	J. i. Oc. D.
	15 56	J. i. Ec. R.
3	5 39	J. ii. Tr. E.
	10 37	J. i. Tr. I.
	12 52	J. i. Tr. E.
	18 5	B.A.C. 4896
		Oc. D. 65° .
4	7 21	J. iii. Oc. D.
	7 44	J. i. Oc. D.
	10 25	J. i. Ec. R.
	11 45	J. iii. Ec. R.
	17 10	κ Libræ
		Oc. D. 106° .
	18 12	κ Libræ
		Oc. R. 223° .
	5 7 18	J. i. Tr. E.
	6 16 3	J. ii. Tr. I.
	8 11 1	J. ii. Oc. D.
	14 48	J. ii. Ec. R.
	9 15 3	J. i. Oc. D.
10	5 12	J. ii. Tr. I.
	7 56	J. ii. Tr. E.
	12 22	J. i. Tr. I.
	14 37	J. i. Tr. E.
11	6 6	ϵ' Capricorni
		Oc. D. 57° .
	6 39	ϵ' Capricorni
		Oc. R. 350° .
	9 30	J. i. Oc. D.
	10 41	J. iii. Oc. D.
	12 20	J. i. Ec. R.
	15 46	J. iii. Ec. R.
12	6 0	κ Aquarii
		Oc. R. 299° .

G. M. T.		
h m		
Jan. 12	6 48	J. i. Tr. I.
	9 3	J. i. Tr. E.
	13 6 49	J. i. Ec. R.
	6 53 15	Piscium
		Oc. D. 124° .
	7 57 15	Piscium
		Oc. R. 285° .
	15 13 18	J. ii. Oc. D.
16	13 4 29	Arietis
		Oc. D. 54° .
	16 49	J. i. Oc. D.
17	7 31	J. ii. Tr. I.
	10 15	J. ii. Tr. E.
	14 7	J. i. Tr. I.
	16 23	J. i. Tr. E.
18	11 16	J. i. Oc. D.
	14 3	J. iii. Oc. D.
	14 15	J. i. Ec. R.
	16 42	J. iii. Oc. R.
19	6 41	J. ii. Ec. R.
	8 34	J. i. Tr. I.
	10 49	J. i. Tr. E.
	11 0	B.A.C. 1651
		Oc. D. 44° .
	19 11 54	B.A.C. 1651
		Oc. R. 310° .
20	5 43	J. i. Oc. D.
	8 44	J. i. Ec. R.
	9 23 71	Orionis
		Oc. D. 15° .
	9 49 71	Orionis
		Oc. R. 336° .
22	6 35	J. iii. Tr. E.
	15 36	J. ii. Oc. D.
23	11 6 60	Canceri
		Oc. R. 253° .
	17 39	κ Canceri
		Oc. D. 139° .
24	9 52	J. ii. Tr. I.

Phenomena (continued).

G. M. T.			
h	m		
Jan. 24	12 37	J. ii. Tr. E.	
	15 54	J. i. Tr. I.	
25	13 3	J. i. Oc. D.	
	16 11	J. i. Ec. R.	
26	9 17	J. ii. Ec. R.	
	10 21	J. i. Tr. I.	
	12 36	J. i. Tr. E.	
27	7 30	J. i. Oc. D.	

G. M. T.			
h	m		
Jan. 27	10 40	J. i. Ec. R.	
	28 7 3	J. i. Tr. E.	
	16 21	ψ Virginis	
		Oc. R. 254°.	
29	7 25	J. iii. Tr. I.	
	10 5	J. iii. Tr. E.	
31	12 16	J. ii. Tr. I.	
	15 1	J. ii. Tr. E.	

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

ERRATA IN VOL. V. No. 68.

Page 382, line 35, *for* derivations *read* deviations.

Page 382, line 46, *for* Parabola $\Delta\delta = +76''$ *read* Parabola $\Delta\delta = +96''$.

Page 386, line 44, *for* in conjunction with *read* in opposition to.

Cover, and pages 362 and 363, *for* Herémy *read* Herény.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 70.

FEBRUARY 1.

1888

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1883, January 12.

E. J. STONE, M.A., F.R.S., *President*, in the Chair.

Secretaries: J. W. L. GLAISHER, M.A., F.R.S., and
E. B. KNOBEL.

THE Minutes of the last Meeting were read and confirmed.

Mr. Knobel announced that 77 presents had been received since the last meeting of the Society. Amongst these were a series of 20 zodiacal charts, by Dr. Peters, which embraced the stars down to the 14th magnitude. Dr. Gill had sent some further photographs of the Great Comet which had been taken at the Cape Observatory with an ordinary photographic camera, mounted equatorially and driven by clockwork. These photographs showed much interesting detail in the comet and its tail, and a large number of small stars around it. There was also a work by Prof. Oppolzer, on the determination of the orbits of comets and planets.

The President. Probably Mr. Hind might like to express an opinion on the charts presented by Prof. Peters, as they are very much in his own line of work.

Mr. Hind. These twenty charts, prepared by Prof. Peters are, I believe, the first instalment of a most arduous undertaking. They are intended to contain all stars down to the 14th magnitude, so far as 30 degrees on each side of the equator throughout the whole of the 24 hours. (Applause.) That is certainly a very great work. Prof. Peters mentions, in a note accompanying the charts, that he has done the whole of the observing and drafting himself, and that the charts are to be published solely at his expense for gratuitous distribution. (Applause.) They will be thus chiefly circulated among those persons who are now engaged in similar work, and will doubtless soon become very scarce. I am sorry there-

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fore that these valuable productions are not to be published and sold in the ordinary way; and for my own part I should be glad if Prof. Peters could be induced to withdraw the conditions under which he is publishing these charts, and give us all the opportunity of providing ourselves with as many copies as we wish. From my own experience, I may venture to say that a single copy of charts like these would be liable to be very soon spoiled.

The President. Among the other presents is a large collection of photographs of the Comet. These have been taken at the Cape of Good Hope Observatory; they are very fine indeed, and I hope the Fellows of the Society will avail themselves of the opportunity of inspecting them, and will see what success has been obtained in that direction.

Mr. Glaisher. A number of small papers have been received on observations of the Transit of Venus made by the Rev. S. J. Johnson at Marseilles; by Mr. Frederic Brodie at Fernhill, Isle of Wight; by the Rev. R. P. Davis at Hatherop, Gloucestershire; by Mr. Horner at Bath; and by M. P. F. Denza of the Observatory of Moncalieri. A printed account has been received of the Transit as observed by Mr. Gill at the Cape of Good Hope, and Mr. J. R. Walker sends us an observation made on board H.M.S. 'Northampton' at Antigua.

The President. The miscellaneous papers on the Transit of Venus will all be published; but until they are discussed it will be impossible to ascertain their full value. We have the pleasure of seeing present this evening one of our most distinguished foreign Associates, Prof. Newcomb [Applause], who has recently been to the Cape of Good Hope in command of the American Expedition that went out to make observations of the Transit of Venus. It is very probable that he will not be in a position, nor care, to give much information about the results of his mission until they have been properly submitted to the Government and discussed; but still any remarks, which he may feel prepared to offer will, I am sure, be gladly heard by the Fellows of this Society this evening. (Applause.)

Prof. Newcomb. The rules which govern the different expeditions for the observation of the Transit of Venus prohibit their members from making any publications of their results; but at the same time, while respecting that prohibition, I shall be happy to give an informal and general account such as we are allowed to communicate to any one who may be interested in the matter. (Hear, hear.) It is perhaps known to Fellows of this Society, who may have read what I have said on this subject during the last few years, that I am not among those who lay very great stress on observations of the Transit of Venus as a means of determining the solar parallax. I do not know that my ideas differ from those generally accepted as to the actual and absolute amount of error in that method; but there are other methods, especially the physical method, by means of measuring the velocity of light, in which I have, from time to time, been engaged during the past

three years which seem to me to afford a much more reliable determination. When, a year and a half ago, it was proposed that expeditions should be sent out to make observations of this Transit, my own opinion was that while we should be prepared to make the fullest observations in our own country (America), where the whole Transit would be visible, it was not worth while to send out any foreign expeditions. When, however, the opinions of the whole of the astronomers of the nation were compared, I found I was in a minority, not exactly of one, but of two; and I submitted to the majority. As soon as the expeditions were finally organized, I offered to take part in one of them, for this reason chiefly. It had been, and still is, my intention to attempt a general discussion of the whole of the observations; and in view of the very great diversity, I might say the irreconcilable discordances, remarked in the description given by different observers as to the phenomena seen in the Transit, I was desirous to see for myself, under the most favourable circumstances, what a Transit really was, and what were the phenomena actually exhibited. I knew in a general way that the climate of South Africa, compared with that of other countries, was in every way favourable during the month of December; but not until I got out there was I aware how good it was. I therefore accepted the charge of the American Expedition to South Africa. Just before setting out I communicated with H.M.'s Astronomer at the Cape, and Mr. Gill forwarded the object of my expedition in every way that was possible. (Applause.) With similar courtesy, your distinguished President answered my applications for information as to the weather at the Cape, thus leaving on my mind the agreeable impression that Her Majesty selects particularly obliging subjects to act as her Astronomers at the Cape. (Laughter and applause.) I arrived in safety at the Cape; and I was advised to select Beaufort, a town in the interior, and 300 miles from the Cape, where cloudy weather was almost entirely unknown in the month of December. I was rather loth to go so great a distance from my base of operations at the Cape; and hearing rumours of clouds and dust and afternoon thunderstorms, I thought it was better not to travel so far, and finally chose Wellington, where the climate, contrasted with our own, we should consider unobjectionable. There we had the advantage of a sheltered situation, clouds of dust were not at all common, and severe winds were nearly unknown. An American Female Seminary afforded me what I most wanted—a position where I might have that seclusion which, for observations of this sort, was absolutely necessary. I was certainly charmed with the climate of South Africa. So far as the different observations of the Sun are concerned, not a particle of vapour obscured the sky. Nor were we subject to the clouds of dust and other inconveniences experienced by observers who went into the far interior.

The day of the Transit came, and although in the morning the

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definition of the Sun was somewhat bad, yet in the afternoon it gradually improved; and, I think, during the Transit, which commenced about 3 o'clock in the afternoon, the definition of the Sun was as fine as I had ever seen it in my life. This was to me a special source of gratification, because I saw the Transit under the most favourable conditions. With regard to physical phenomena, there was only one worthy of note, and that one was so well marked that it could not escape observation. It was a line of light which surrounded the hemisphere of Venus, which was off the Sun. I looked very carefully at the first contact to see whether it was possible to see Venus projected on the corona; but I was unable to see any sign of the planet until the time of the first contact. At the time of the first contact there was absolutely nothing to be seen but the geometrically cutting out of the solar disk. In the words of Lieut. Casey, my assistant, it seemed as sharp and well defined as if a piece of the Sun had been cut out with a knife. Just as the planet was half on the Sun, that line of light, which has been described by so many observers, and which I looked for in vain before, began to show itself. It was, however, not continuous around the planet, but was seen only at a certain point. A phenomenon was now approaching, as to the exact nature of which observers had differed, and for which therefore I looked with the utmost solicitude. This line of light, due to the atmosphere of Venus, would soon appear to unite the cusps. Could it be distinguished from true sunlight appearing behind the planet? As the contact approached I observed that the line of light became slightly brighter, and some seconds before the internal contact it was continuously visible. At the time to which I allude it appeared equally intense throughout its whole length, whereas the cusps, from the natural effects of irradiation, were a little blunted. I observed that the cusps were gradually dissolved in this line of light until they could no longer be distinguished; but I noticed, during an interval of five seconds following this time, that the light behind the planet did not increase in amount. On the contrary, there was what observers have called a "hesitation;" that is, for a period of about five seconds, during which there was no apparent change of character, it was impossible to determine whether the contact had passed or not. But at the end of the five seconds the light began to flow in rapidly. There were these five seconds of indetermination and uncertainty, and I suppose we shall take the mean of that period as the time of true contact.

The only point in which the phenomenon in the present contact differed from the model contact was in regard to this atmospheric line. The model contact, in 1874, was not, as is commonly supposed, purely geometrical; but in order to secure the most perfect representation of the actual phenomenon the model was placed at a distance of nearly half a mile, so that the atmospheric softening might be produced. Moreover, Venus was not made perfectly dark, and we endeavoured, as nearly as possible, to

make a perfect representation of the actual Transit. The only difficulty was the presence of this line of light. In some experiments I had painted a narrow white line on the model of the planet, in order to represent the bright line which would exist. In the absence, however, of any definite knowledge on the subject, I had not laid sufficient stress upon the possible value of the observations of this line of light.

The President. After the contact took place, was there, between the limbs of Venus and the Sun, a light equal to sunlight, or was there something in the nature of a shade?

Prof. Newcomb. I did not observe any thing in the nature of a shade. I have, however, seen this shade in the model Transit, and it was obviously due to irradiation. The definition was so very sharp and so good in the case of the actual Transit, that I did not see any appearance of a shade; but I do not lay any stress upon that matter. I am not sure, but it might happen, in ordinary observations, if there was sufficient atmospheric tremulousness, that the softening of the light would produce a shade.

The President. I am sure we have all listened to Prof. Newcomb's remarks with very great pleasure and interest. If observations can be made with any thing like the accuracy he has indicated, within, that is, two or three seconds, we shall be able to obtain exceedingly accurate results from such observations as those made at Bermuda as compared with those made at Madagascar and the Cape. But the difficulty, so far as I can ascertain it, is in the subsidiary phenomena. The kind of contact to which Prof. Newcomb referred is, no doubt, the contact to be observed; but a great number of the observers of the Transit of Venus have noticed, after that, something like a faint shade. And when observers see such a thing as that, some hold to it and consider it important, while others do not; and you thus increase the liability to error of your observations, which error is measured by the differences of the observed times if you consider both to refer to the same phenomenon. As we have a good number of observers in each district, we ought, I think, by taking the mean of their observations, to arrive at an accurate result, for the mean phase of all our northern observers should not differ much from the mean phase of all our southern observers. Considerable stress has been laid by Prof. Newcomb and other astronomers on the method of determining the solar parallax by means of the velocity of light. I am inclined to believe that we are able, or soon shall be able, to determine the velocity of light, as observed on the Earth's surface, with great accuracy; but there is a step to be made between that and the determination of the Sun's distance. We have to assume a definite relationship between the velocity of light, the velocity of the Earth in its orbit, and the coefficient of aberration before we can determine the solar parallax from the velocity of light; but do we know, as a matter of fact, that that relationship is exact? When the undulatory theory was on its trial I believe one of the great objections to it was that it

afforded no satisfactory explanation of the theory of aberration. It was shown, however, by Fresnel that the objection was not a fatal one; and he proved that upon an hypothesis which, although improbable, was not inconceivable, the undulatory theory could be made to account for aberration; but certainly, as the hypothesis adopted can hardly be considered by any one as a physical truth, Fresnel did not prove that the relationship was exact.

Many years after, Professor Stokes showed that, upon an assumption of greater probability, the common assumed relationship between the velocity of the Earth, the velocity of light, and the coefficient of aberration could be deduced from the undulatory theory. But I should doubt whether Professor Stokes can be considered to have proved the exact truth of the assumed relation between these quantities. He has assumed, to integrate his equations, that $u dx + v dy + w dz$ is a perfect differential, and to deduce the relationship, that u , v , and w are the components of the velocity of the observer due to the motion of the Earth in its orbit. As the ether has been assumed to be at relative rest near the Earth's surface, and the motion of the Earth in its orbit is not a perfect differential, these assumptions hardly appear to me strictly consistent with each other. Although, therefore, there is no doubt the ordinary assumed relationship is very approximately true, we have no proof that the relationship is exact within the errors with which we are concerned; and until this has been proved, the deduction of the Solar Parallax from the assumed relationship appears hardly sound, although the close agreement between the results thus obtained may be interesting as showing how nearly correct the assumed relationship must be.

I merely call attention to the matter by way of caution. I quite admit that the method of determining the solar parallax in the Transit of Venus is not a perfect method; in fact it takes twenty seconds of time to effect a separation of 1" of arc of Venus from the Sun's limb. After all, these observed contacts are nothing more than observations, and we must expect to have errors probably of half a second of arc, and we cannot therefore expect to find the parallax to 0".02. The difficulty seems, however, to be equally great in regard to the planetary method of determining the solar parallax.

M. Tisserand got a value 8".84 from Le Verrier's equations of condition for the motion of Venus, which he has cleared from the unknown mass of Mars, now determined directly from the observations of the satellites; but he has also got 9".01 for the equations for the motion of Mars; although he has, upon a probable supposition, reduced this value to 8".92. Still these values do not agree perfectly, although based upon observations extending over a hundred years. This shows the extreme difficulty of avoiding the effects of systematic errors in such work when we wish to fix the parallax to the second place of decimals. There is one point connected with M. Tisserand's papers which appears to me to require reconsider-

ation. M. Tisserand has reduced the value $8''.84$, deduced from the Venus observations, to $8''.74$, by a process of great refinement, but which, I venture to think, is scarcely satisfactory. The method followed is that an unknown quantity, ϵ , is introduced into the equation, expressing the change of obliquity as due to a supposed error in the determinations of Bradley and Maskelyne; and then leaving all the other equations of condition unaffected by the supposed error, the *three* equations for the determination of the masses of Mercury, Venus, and the Earth are solved, and the corrections to the assumed values of these masses expressed in terms of known quantities and the supposed unknown correction due to the error of Bradley's obliquity.

The masses thus found are then carried into the equations of condition for the motion of the Earth, all still uncorrected for the supposed error, ϵ , except that for the change of obliquity. The substitution thus leaves a series of equations of condition involving ϵ , which is found by the method of least squares; and the correction thus found, applied to the determination of the Earth's mass, gives the value of the parallax $8''.74$. All this would be satisfactory enough if *none* of the equations of condition would be affected if such an error as ϵ did exist in Bradley and Maskelyne's determination of the obliquity. But such appears to me not to be the case. The geocentric latitudes and longitudes must be reduced with the obliquity obtained with the assumed correction on one uniform system before the equations can be thus treated. From a trial I have made, the effects of an error of ϵ upon some of the equations of condition would be of the same order as that of ϵ .

In heliometer-measures, so far as I have yet had an opportunity of examining them, there is also one point which throws, to some extent, a doubt upon the accuracy of the deduced result, although the observations themselves are of extreme accuracy. To take the strongest case, when the distance of a planet from two stars, one of greater and the other of less right ascension, is measured east and west of the meridian; by comparing the distance of each star from the planet as measured on the two sides of the meridian, we can get a determination of the parallax affected with no systematic errors, but corrections for refraction, motion of planet in interval, and small error due to error of scale only over the *change* of distance measured. The result should therefore be an accurate one. But by taking each star we ought to get independent determinations; and if these agreed very closely, it would prove that any systematic errors must have been reduced to very small limits. When, however, we have to make the results from the two stars agree by the supposition of a change of scale-value, although the result deduced may be true, yet its truth is not apparent from the equations: the check is lost. The method in one case is a very strong one indeed; but in the case of the necessity of supposing different scale-values east and west of the meridian, I must confess that I consider its strength problematical.

As the result of my work on this subject, I am inclined to think that none of our methods can be considered as free from suspicion of systematic errors; and I much prefer therefore to get results from as many independent methods as possible, and trust rather to a mean result than to one obtained from any one method, however small the theoretical probable error may be considered. If we could once settle, within small limits, what the true value of the Solar Parallax is, we should then be able to re-examine all the methods which had led to somewhat discordant results, and an increase of accuracy in these methods would be almost certain to result. (Applause.)

Prof. Newcomb. I agree with almost every thing the President has said with regard to the objections which have been made to the application of the theory of aberration in the determination of solar parallax. I have carefully considered them, and the strongest appears to be that of Lord Rayleigh's, viz. that we cannot be sure of the velocity obtained by measure being the actual velocity of the light-waves. The difficulty he mentions is one that can only be settled by general reasoning. The next objection to the theory of aberration is the possibility of the motion of the ether. So far as my knowledge extends, no one has yet pointed out any optical phenomenon which may be considered an effect of the material existence of the ether or possible motion of the ether in a void space. In other words, if we leave out of consideration some experiments of certain French physicists as to the motion of the material medium through which light passes, all the phenomena of aberration depend solely upon the relations or motions of the luminous point and the observer. No optical phenomenon has yet been discovered in which an observer at *O* can see any difference in a phenomenon at *E*, in consequence of the motion of the two objects in one direction. We have no direct proof of ether having any material existence in space, as shown by the phenomenon of light. It is one of the great optical *desiderata* to ascertain whether the ether is at rest or is moving at great velocity. The only approximation to such a suggestion is one made by Professor Maxwell for observation of the satellites of Jupiter.

The President. With regard to the differential solution of the equation, Professor Stokes assumed that $\frac{dv}{dz} = \frac{dw}{dy}$. It is on these assumptions that the solution is based; and these assumptions, I maintain, are not absolutely true, when the motion of the Earth in its orbit is taken into consideration, for ether in relative rest to the Earth.

Mr. Christie. Is that not rather an objection to the undulatory theory in relation to the aberration difficulty than to the question of aberration itself? The question of aberration does not rest upon that explanation of it, but, rather, the undulatory theory has to explain the fact of aberration, which is an observed fact. The matter is an intricate one; but, I think, the President has stated

the case rather strongly when he says that these two facts are irreconcilable. Professor Stokes takes account of this in his explanation; but, as I only speak from recollection, I cannot go into details. He, however, presses the point, that at some distance from the Earth this quantity would naturally be a perfect differential. That is a condition which holds in the motion of all perfect fluids. And the Earth carries its own ether with it in its motion. But there is between the two an intermediate condition of things which would not sensibly affect the phenomenon of aberration. That is what I gathered from the explanation of Prof. Stokes, that the effect upon the phenomenon of aberration was merely infinitesimal.

The President. The solution is effected upon those conditions being satisfied, and the ether is supposed to be at rest relative to the observer. I do not mean to say that the relationship may not be explainable upon some other hypothesis; but I do say that the theory of aberration has been considered a difficulty with regard to the undulatory theory of optics. There are two explanations respecting it—one of Fresnel and one of Stokes, neither of which appears complete in itself. The assumed relationship is not established as an absolute matter of fact.

Mr. Christie. It is not absolute, but it is true to a very high degree of approximation. My point is, that this difficulty affects rather the undulatory theory than the value of the constant of aberration.

The President. How would the connexion be affected between the velocity of the Earth in her orbit and the velocity of light?

Mr. Christie. I do not see how any thing of the sort could increase aberration*.

Prof. Adams. It appears to me that we may look upon the undulatory theory as perfectly established. Therefore, every thing, in order to be sustained, ought to be consistent with that theory. As I understand the President, he does not deny that the undulatory theory would give the coefficient of aberration very approximately such as we actually observe it to be; but he means that, in accurate observations, you may possibly find an actual minute difference between the velocity of light as determined by experiment and the velocity of light which would be given by the observed value of the coefficient of aberration. The only question is, what is the order of the small quantities neglected in the theory? Upon that I confess I do not pretend to form an idea.

The President. Professor Adams has exactly stated my position with regard to the point before us; I do not pretend at all to say what degree of difference there may be. In regard to the value of the solar parallax, as obtained from the velocity of light, the difference from the values obtained by other methods is so slight that I am rather astonished at the agreement. The method may therefore

* If the motion of the ether had any sensible effect, it would only be to diminish aberration, so that the deduced velocity of the Earth in her orbit would be too small and the parallax too large.

be exact; but, until the exact relationship has been established, you cannot use it to discriminate between the minute differences of values of the solar parallax deduced by different methods.

Mr. Knobel read a communication from *Professor Langley* relative to the Transit of Venus, in which Prof. Langley described a notable gathering of brightness on the limb of Venus a few minutes before internal contact. This bright patch was about 30° on one side of the line joining the centres of the Sun and planet, and seemed to extend within the limb of the planet, but was brightest at the limb, was about one quarter the planet's radius in breadth, and had a bright star-like point in its centre. The paper was accompanied by a drawing. The phenomenon was watched for 7 or 8 minutes, both by Professor Langley and by his assistant.

The President. I would ask Professor Newcomb whether the position in which Professor Langley places the spot, on the limb of the planet, agrees accurately with what he observed.

Prof. Newcomb. Prof. Langley's broadening of the line is exactly where the line appeared brightest to me, but I saw no broadening.

The President. In the Transit of 1874 I saw a spot, but it was after Venus was wholly on the Sun's disk, similar in character to that represented by Prof. Langley, but rather brighter.

Mr. Horner. What I saw was rather a hazy light, and not of the character of a ring. It was difficult to localize; it was diffused all round the limb. I saw it first without a shade. It would be interesting, therefore, to know what sort of tint Prof. Langley used in his dark glass.

The President. That is not stated. I am sure we shall all return our thanks to Prof. Langley for his interesting communication. (Applause.)

Mr. Knobel read a communication by *Mr. Prince* on "Physical Observations of the Great Comet," and another on the same subject by *Mr. B. J. Hopkins*. Some sketches made at Mussooree, N.W. Provinces, India, were also exhibited.

The President remarked that nearly all these observations of the comet seemed to show that it was composed of a series of bodies not very far apart, a circumstance which would explain the remarkable observation made at the Cape, where the comet was seen close up to the Sun's limb, but was there lost.

Prof. Adams. According to a letter received from South America, the comet was seen just before perihelion very close to the Sun, and the brightness seemed to be equal to that of the Sun. That may explain how it was lost sight of when it got on the Sun. The account to which I refer is a rather striking one. On the one day the comet was seen rapidly approaching the Sun, and the next day the observers expected it to come out on the other side; but, instead of that, it came out on the same side. (Laughter.) It had described 180° in the interval.

Prof. Adams read a short note upon William Ball's observation of Saturn's ring. In No. 9, Vol. i. of the 'Philosophical Transac-

tions,' a brief account was given of an observation of Saturn made on Oct. 13, 1665, by William Ball, at Mamhead, near Exeter. This account had recently given rise to some discussion, and there were some difficulties connected with it which did not appear to have been satisfactorily cleared up. In a few copies of the volume the account was illustrated by a figure of the external boundary of the ring, and instead of being of a regular elliptical form it had two blunt notches at the extremities of the minor axis. No telescope capable of showing Saturn's ring at all would have exhibited it in that extraordinary form. Therefore, if the figure thus depicted faithfully represented Ball's drawing, he was either a very inaccurate or a careless observer, or else he must have been provided with very inadequate instruments. On the other hand, there was ample proof that he was a careful and assiduous observer, and that he made a long series of observations of Saturn with instruments not much inferior to those employed by Huygens himself. Huygens' discovery with regard to Saturn was disputed by Father Fabri at Rome. In a tract contained in the third volume of his collected works, he repeatedly appeals to Ball's observations in England in confirmation of his own. And it is clear that Huygens was in possession of drawings by Ball, which represented the various appearances of the planet during the years 1656-59 inclusive, and had compared them with his own taken during the same interval. Having mentioned the dark band he had observed on the ball of Saturn when the ring was invisible, he quotes a letter of Dr. Wallis's, in which reference was made to this band as having been observed by Ball, and Dr. Wallis inquired whether his correspondent had likewise perceived it. This was before the publication of Huygens' own book. Huygens went on to say that from the 5th of Feb., 1656, to the 2nd of July, when the planet appeared round, this band or dark shade was observed by Ball across the centre of the disk and shown in the drawing exactly as in Huygens' own figure. From Nov. 5, 1656, to July 9, 1657, when the oblong arms of Saturn were seen united to the disk, Ball gave a figure similar to that depicted by Huygens, except that he made the arms a little bigger. Having made these comparisons, Huygens remarked that Ball was unacquainted with his hypothesis respecting the ring, and therefore could not be supposed to be biased by it, while he himself would not dare to represent the phenomena otherwise than they really were, since, if he did, he would be contradicted by the English observers. This judgment on the part of so competent an authority as Huygens leaves no doubt as to the merit of Ball's observations. In order to see whether any further light could be thrown on the subject, Prof. Adams went on to say that he had consulted MSS. in the archives of the Royal Society, and among them a letter in Ball's hand, dated April 14th, 1666, in which he stated that his observations of Saturn were made partly with a telescope 38 feet long and a double eyeglass, and partly with a telescope 12 feet in length.

In the postscript he then gave a small pen-and-ink sketch of Saturn. That was in 1666; and he remarked that the same appearance was presented by the planet in 1664. In this figure the external boundary of the ring had the form of a regular oval, without any notches or other irregularities. No allusion was made to the very different appearance which, if the figure in the 'Philosophical Transactions' is authentic, the planet must have presented in 1665. The paper in the 'Philosophical Transactions' was not written by Ball himself, but it contains a quotation from a letter of Ball to a friend; and in what appears to be the last clause of this quotation, the figure is said to be "a little hollow above and below." I cannot help thinking that this clause has been added or altered in some way to correspond with the given figure. The letter of Ball, upon which the paper was founded, is not in the archives, but there is preserved a paper cutting representing the planet and his ring. The defect in the paper cutting probably originated in the following way: in order to make a cutting, the paper was folded twice in directions at right angles to each other, so that only a quadrant of the ellipse had to be cut; but, through want of care, when the cutting began at the minor axis its direction formed a slightly obtuse angle with the axis instead of being perpendicular to it. Consequently, when the paper was unfolded, notches appeared at the extremity of the minor axis. The account, I think, must have been written by some one inexperienced in astronomical observations, who took for granted that the figure was correct. When the mistake was discovered, the erroneous plate would seem to have been cancelled; and hence its appearance in only a few copies. The other figures in the cancelled plate were repeated in a new plate, which accompanied No. 24 of the same volume of the 'Transactions.' That repetition of the figures minus the error is a pretty clear proof that the cancelling of the former plate was done designedly.

Mr. Common. I have one remark to make on the subject of Prof. Adams's paper. When the discussion on this question appeared in the periodicals some time ago, I was engaged in taking photographs of Saturn, and on examining them I found a peculiar effect. According to the ordinary representation, Saturn ought to show, in photographs, as a perfect oval; but the effect produced in the various photographs, particularly those exposed the longest time, was this: where the ball was in front of the ring there was no action on the plate; where the ring itself was free to work there was no irradiation; giving the peculiar notched appearance, as in Ball's drawing, but on one side only. Supposing the ring opened to the same extent, and viewed through a bad telescope, there might be a similar notched appearance to the eye, giving rise to the impression of there being two separate rings.

Prof. Adams. Did you find that these cusps occurred both above and below the planet?

Mr. Common. No.

Prof. Adams. The two sides of the ring are exactly symmetrical in the drawing in the 'Transactions.'

Capt. Noble. I believe there is an engraving by Huygens himself in which the same notches are shown, but very much exaggerated.

Prof. Adams. Not by Huygens, but by Father Fabri, whom Huygens quotes in order to confute.

Capt. Noble. The discussion originally raised turned on the point whether Ball was rightly or wrongly credited with being the discoverer of the black division in the ring. Ball, no doubt, was as accurate as observers in those days could be with their imperfect instruments; but the real point was, had he been rightly or wrongly credited, for the last forty or fifty years, in astronomical works with being the discoverer of the primary division?

Prof. Adams. No: there is no question about that point.

Capt. Noble. If I remember correctly, it is stated in the 'Philosophical Transactions' that a letter was written to Huygens asking him to look at the planet and see whether there was not something which would induce him to imagine that Saturn was surrounded, not by one ring, but by two.

Prof. Adams. Such a letter as you refer to appears to have been written in 1665, but not by Ball; whereas the letter I referred to was written in 1656, nine years earlier. The question put to Huygens by Wallis was in 1656. Saturn appeared without any ring at all, and an observer near Exeter noticed an obscure band crossing the centre of the planet. Wallis communicated the fact to Huygens, and asked if he had seen any thing like it. Two years afterwards Ball had carried on his observations, and Wallis wrote to Huygens again, reminding him of the former letter in which he had spoken of the same matter. But by that time the ring appeared to have been gradually opening out, and no doubt Ball had sent him drawings corresponding not only to the first year, but to the other two years, and then again to the year after that; so that when Huygens wrote, he had, as he states in his own paper, drawings of Saturn made by Ball in four consecutive years, and showed how well his drawings corresponded with them; in fact, he demolished his opponents by bringing Ball to bear upon them.

The President. There seems no doubt that Prof. Adams's explanation appears perfectly satisfactory.

Capt. Noble. But why did Ball get credit for the division at all?

Prof. Adams. That is a mistake. He never claimed credit for it.

The President. I should like to ask Mr. Common to pass round his photograph of the nebula of Orion. The photographs of this object have been increasing in beauty of late years. He has one which is by far the best I have ever seen, and I should like the Fellows of the Society to look at it. (Applause.)

Mr. Rand Capron. With reference to the photographic question, I some years ago tried to photograph Saturn. When I came to examine the result I thought I had got two images, but on closer

examination I discovered that the figure was exactly that given by Mr. Common.

The following papers were also announced :—

A. Marth. "Ephemeris of the Satellites of Uranus, 1883."

Prof. R. Grant. "Observations of the Transit of Venus, 1882, Dec. 6."

F. Brodie. "Observations of the Solar Spots of November 1882."

W. F. Denning. "The Aquariads of April 29 to May 3."

Capt. G. F. Parson. "Sextant Observations of the Great Comet (b) 1882, made on board the ship 'Earnock.'"

Capt. D. W. Barker. "Sextant Observations of the Great Comet (b) 1882, made on board the ship 'Superb.'"

G. A. Goldney. "Observations of the Phenomena of Jupiter's Satellites, made at the University Observatory, Durham, in the year 1882."

Prof. C. Pritchard. "Postscript on a communication made to the Royal Astronomical Society in November last, on Stellar Photometry."

W. F. Denning. "Observations on Jupiter."

L. A. Eddie. "The Great Comet (b) 1882."

E. J. Stone. "Reduction of Latitude and Logarithm of the Earth's Radius with Col. Clarke's Value of the Earth's Compression."

R. A. Proctor. "Meteors and Meteorology."

Prof. J. A. C. Oudemans. "Note upon the Longitudes of Madras, Singapore, and Batavia."

Astronomer Royal. "Spectroscopic Results for the Motions of Stars in the Line of Sight obtained at the Royal Observatory, Greenwich, in the Year 1882. No. VI."

Astronomer Royal. "The Spectrum of the Great Sun-spot of 1882, November 12-25, observed at the Royal Observatory, Greenwich."

The following gentlemen were duly elected Fellows:—F. C. Green, Dr. A. H. Howe, Rev. J. C. Lunn, S. T. H. Saunders, E. J. Spitta, W. H. Walmsley.

The Meeting adjourned at 10^h 0^m.

A Plan for securing Observations of the Variable Stars.

For several reasons the investigations here proposed are especially suited to observers under very various conditions. The work is capable of indefinite subdivision. Small as well as large telescopes may be employed and many observations are needed which can best be made with an opera-glass or field-glass, or even with the naked eye. No attachment is needed to an ordinary telescope, so that no additional expense on this account is required. Useful observations may be made by an unskilled observer pro-

vided that he is capable of identifying a star with certainty. The work is quantitative, and the observer has therefore a continual test of the increased accuracy he has acquired by practice. As a portion of the investigation will probably lead to the discovery of interesting objects, the observations will possess an interest often wanting in quantitative research. The aid of the professional astronomer is earnestly requested for this scheme. Suggestions by which it may be modified and improved will be gratefully received. The professional astronomer, in consequence of his greater skill, instrumental appliances, and command of his own time, could fill gaps in the work, and thus greatly increase its value as a whole. Such observations could often be made in the intervals of other work or at times unsuitable for the observations to which he was especially devoting himself. It should be added that especial care will be taken not to interfere with observations of variable stars now in progress. Observers of these objects are particularly requested to notify the writer what work they propose to carry out, so that a needless repetition of it may be avoided.

It is on the amateur and student of astronomy that we must depend largely for the success of the plan here proposed. Many such persons spend evening after evening at their telescopes without obtaining results of any permanent value. Either no publication is made and the results are therefore valueless, or time is spent on objects that can be much more usefully examined with a larger instrument. Most commonly the observer has no special plan and spends many hours without result, while the same time might have been employed with equal pleasure to himself and results of great value collected. Those who have not tried it do not realize the growing interest in a systematic research and the satisfaction in feeling that by one's own labours the sum of human knowledge has been increased.

Much valuable assistance might be rendered by a class whose aid in such work has usually been overlooked. Many ladies are interested in astronomy and own telescopes, but with two or three noteworthy exceptions their contributions to the science have been almost nothing. Many of them have the time and inclination for such work, and especially among the graduates of women's colleges are many who have had abundant training to make excellent observers. As the work may be done at home, even from an open window, provided the room has the temperature of the outer air, there seems to be no reason why they should not thus make an advantageous use of their skill. It is believed that it is only necessary to point the way to secure most valuable assistance. The criticism is often made by the opponents of the higher education of women that, while they are capable of following others as far as men can, they originate almost nothing, so that human knowledge is not advanced by their work. This reproach would be well answered could we point to a long series of such observations as are detailed below, made by women observers.

Variable stars may be defined as those which exhibit a varying degree of brightness at different times. The following classification of them is believed to be a natural one. (Proc. Amer. Acad. xvi., 1, 257.)

I. Temporary stars, or those which shine out suddenly, sometimes with great brilliancy, and gradually fade away. Examples, Tycho Brahe's star of 1572, new star in Corona, 1866.

II. Long-period variables, or those undergoing great variations of light, the changes recurring in periods of several months. Examples, α *Ceti* and χ *Ceti*.

III. Stars undergoing slight changes according to laws as yet unknown. Examples, α *Orionis* and α *Cassiopeiae*.

IV. Short-period variables, or stars whose light is continually varying, but the changes are repeated with great regularity in a period not exceeding a few days. Examples, β *Lyræ* and δ *Cephei*.

V. Algol stars, or stars which for the greater portion of the time undergo no change in light, but every few days suffer a remarkable diminution in light for a few hours. This phenomenon recurs with such regularity that the interval between successive minima may be determined in some cases within a fraction of a second. Examples, β *Persei* (Algol) and δ *Canceri*.

Stars belonging to the first of these classes are seen so rarely that the apparent discovery of one is to be received with the utmost caution. On the other hand, the importance of early observations of such an object is so great that no pains should be spared to secure an early announcement if one is really found. On the best star charts many stars are omitted of the brightness of the faintest objects given. But any star much brighter than these should be measured by the method given below, and a watch kept to see if any change takes place. If it proves to be a temporary star, an immediate announcement should be made. If a telegram is sent to this Observatory the object will be at once examined, and, if verified, notification will be made in this country and in Europe, with the name of the discoverer or sender of the telegram. A similar notification may be sent of any suspected objects, which will be examined in the same way, and announced at once if they prove to be of interest. It is essential that the position of the object should be given with all the precision practicable, and that a letter should be sent by the next mail giving the observations in detail. This often proves of the greatest value in case the object is not readily found. It also serves to establish the claims of the first discoverer.

Nearly three quarters of the known variables belong to the second class. Most of them undergo very large changes of light, and may therefore be observed with comparative ease. Our knowledge of their variations is, however, very defective. Hitherto the attention of observers has been directed principally to determining the times at which they attain their maximum light, while their light at intermediate times has been neglected. It is now pro-

posed to secure observations of these objects once or twice in every month, so that their light curves or variations throughout their entire periods may be determined. Again, many observers are accustomed to state their brightness in magnitudes, without giving any clue to the scale which they employ. In most cases such observations have little value, owing to the uncertainty of the scale of the fainter magnitudes.

According to Dr. Gould and some other observers, most of the visible stars undergo slight changes of light and should therefore be assigned to the third class of variables. It is probable that our Sun also belongs to this class, as it is not likely that its light is the same during the maximum and minimum of the sun-spot period. At present we are unable to tell in which case the light would be greatest. It by no means follows that when the spots are most abundant the Sun's total light is least, for the remaining portions of the Sun may then have an increased brightness more than compensating for their diminished area. As long as the suspected variations in light of the stars are small, not exceeding half a magnitude for instance, they seem in the present state of science to have comparatively little interest. They are so liable to be affected, or even caused, by errors of observation, that the observation of such objects does not seem now to be advisable. Doubtless many such so-called variables are really due to errors caused by moonlight, the proximity of brighter stars, varying position of the images on the retina of the observer, and other similar causes. They will not therefore be considered further in this paper.

The stars of the fourth class as compared with the second are relatively few in number, and the change in light small. While many of them need observation, especially to determine their light curves more precisely, it is advised that this work be left to those who have acquired a high degree of skill in these observations. That the work may be of value it is essential that the errors should be extremely small. As, however, nearly all are visible in an opera-glass, a skilful observer unprovided with a telescope may secure valuable results by their observation. This remark applies with especial force to many of those discovered in the southern heavens by Dr. Gould.

The phenomena of the Algol stars are in many respects the most striking of any. The rapidity of the changes, their surprising regularity, and the comparative rarity of these objects, combine to render the discovery of each new one a matter of unusual interest. As in the case of stars of the fourth class, however, the study of their light curves should be left to those who have acquired especial skill in this work. This is particularly desirable, when, as in this case, the unaided eye enters into competition with photometric apparatus, by which, as some think, it should properly be altogether replaced.

An elaborate bibliographical work on the variable stars has been undertaken at this Observatory by Mr. Chandler. It will include the collection of all available published observations of

known or suspected variables. A catalogue of suspected variables has thus been prepared, doubtless containing many stars which are really important variables. But it is also likely that many objects have been introduced in the list by errors in the original observations. Such stars often appear in one catalogue after another of suspected variables, and it is difficult to prevent the continued circulation of such an error. Of course if an experienced observer at any time estimates a star as above or below its normal brightness, it is impossible to prove that the observation was not correct, and the star really variable. No amount of subsequent observing could prove that it had not then, and then only, an abnormal brightness. We can, however, prove that in all probability it does not belong to one or more of the above classes, and thus make it more and more probable that the observation is due to an error. If the star varies in light by one magnitude, what will be the chances that we shall get a series of observations having a range of variation of one fifth of a magnitude? Evidently on the average, there will be only one chance out of five that any observation shall fall in the same fifth of a magnitude as another. The chances for three such observations will be only $\frac{1}{125}$, and four $\frac{1}{15625}$, &c. These ratios expressed decimally are .2, .04, .008, .0016, .0003, &c. Since the separate determinations of the light of a constant star by the method given below should not differ more than two or three tenths of a magnitude, it is obvious that if the variations of the star are large, a few observations would generally establish this fact. If the star belongs to class four, observations on half a dozen evenings would hardly fail to show the variation. Conversely, if so such variation is detected we may be almost certain that the star is not a variable of that class, or at least that the variation, if any, is not large. If the star belongs to class two, it will change so slowly when near its maximum or minimum that a variation might not be noted if the observations are near together. An interval of several months should therefore be allowed to take place, or perhaps it would be better to wait until the star is again visible the following year. The total variation in light is usually so great in these stars that the change will often be visible at the first glance.

To prove that a star does not belong to the fifth class is a matter of much greater difficulty. In fact it is almost impossible to prove that it may not be an Algol star with a long period between the minima. Since these stars may have their full brightness for nine tenths of the time, it is obvious that they may be examined again and again without happening to be seen at the time of a minimum.

On the other hand, during a considerable portion of the time when it is varying, the light will be so much less than usual that a careful measurement is not needed to detect the change. Moreover, it will be useless to look for an increase of light, and the observation may be so planned as to detect a diminution only.

If we assume that only during one tenth of the time the change in light will be sufficient to be perceptible, the chance on any given evening will be 9 out of 10 or $\frac{9}{10}$ that the star will have its full brightness. For two evenings the chance will be $(\frac{9}{10})^2$, for three $(\frac{9}{10})^3$, &c. These quantities expressed decimally are .9, .81, .73, .66, .59, .53, .48, &c. Even after seven nights' observations, on which no change is noted, it will only be about an even chance that the star may not still be of the Algol type. A different method of observing is therefore recommended when the star is supposed to belong to this class. Select for comparison a star slightly fainter, so that a moment's glance will satisfy the observer that the suspected variable is the brighter. It is only necessary to repeat this observation night after night. If the star is bright enough to be visible with a field-glass, a few seconds will be sufficient for this observation after the observer has become familiar with the vicinity. The fact that the light is normal, and the time to the nearest minute, should be recorded after each observation. When convenient, it is well to repeat the inspection two or more times during the night, as in determining the period all the observations will have a value, provided that they are separated by intervals of more than two or three hours. If the star is ever found below its normal brightness, comparisons should be made with the adjacent stars, and continued as long as possible, or until it has regained its usual brightness. The most complete proof that a star was not of the Algol type would be for observers in the polar regions to examine it at intervals of a few hours for several days, or for observers in different longitudes to make the same observations. If it could thus be watched for a week or fortnight by enough observers to avoid interference by clouds, it would be nearly certain that it is not an Algol star unless its period is greater than that of any such object as yet discovered.

E. C. PICKERING.

[To be continued.]

CORRESPONDENCE.

To the Editor of 'The Observatory.'

On the Calculation of the Corrections arising from the Azimuth, Level, and Collimation Errors of the Transit Instrument.

"As a general rule, when great accuracy is required, it is easier and safer to adjust by computation than by mechanical contrivances."—*On the Principal Mathematical Instruments*, by F. W. SIMMS, F.R.A.S. &c.

SIR,—

It is not proposed in what follows to enter into the investigation of the methods for obtaining the values for the instrumental errors of the Transit Instrument, nor the derivation of the neces-

E 2

sary formulæ for the correction of the time of transit for these errors when known, as the student will find everything he can require on these subjects in the treatises of Chauvenet, Loomis, and Brünnow*. I propose to treat here only a method of facilitating the *calculations* necessary to free the time of transit of a star from the effect of these errors. I am not acquainted with the daily routine of a public observatory, but am induced to think the following may not be altogether uninteresting to some of your amateur readers, for whom alone I venture these remarks. With the exception of the standard article on the Transit Instrument in the 'Penny Cyclopædia,' by the late Rev. R. Sheepshanks, our treatises on Practical Astronomy contain few or scant hints how these necessary calculations may be abbreviated. Loomis and F. W. Simms give examples of the full logarithmic process, in which six logarithms have to be looked out, involving a number of figures, even when only four decimal places are employed. At Greenwich, I believe, a slide rule is used for these subsidiary calculations. I may mention that my instrument is a portable one, by Troughton and Simms, of $2\frac{1}{4}$ inches aperture and 30 inches focal length, almost precisely similar to the one figured in Mr. Sheepshanks's article mentioned above. This figure is reproduced on a smaller scale in the 'Practical Astronomy' by Loomis.

For the reduction of the transit I use the well-known formula:

$$a \sin \frac{(\phi - \delta)}{\cos \delta} + b \frac{\cos (\phi - \delta)}{\cos \delta} + \frac{c}{\cos \delta},$$

or,

$$a \sin (\phi - \delta) \sec \delta + b \cos (\phi - \delta) \sec \delta + c \sec \delta,$$

attributed by Chauvenet and Brünnow to the celebrated Tobias Mayer, in which a is the deviation of the telescope in azimuth; b the inclination of the axis, or the level error; c the collimation error. As these errors affect the seconds of time of the transit of a star, "the unit of the quantities a, b, c should be the second of time," or the seconds of arc divided by 15; ϕ is the latitude of the place of observation, and δ the declination of the observed star; $(\phi - \delta)$ is thus the zenith distance of the star. "Throughout we shall assume the latitude to be north; when the declination is south, the sign of δ is changed, and $(\phi - \delta)$ becomes $(\phi + \delta)$. North of the zenith the numerator is negative, and the correction is to be subtracted between zenith and pole. Below the pole the $\cos \delta$ becomes negative (reckoned through the pole), and the correction becomes additive, as at first." (Sheepshanks, 'Penny Cyclopædia.') Referring to the formula, it will be seen that in any given observatory the trigonometrical functions depend on the declination, and in a given star are only liable to vary by a change in the declination of the star, owing to precession. This change is very small

* In particular, in Chauvenet's 'Spherical and Practical Astronomy,' vol. ii. pp. 140-145, there is a complete and beautiful discussion of the formulæ of the transit instrument by spherical trigonometry.

in a number of years, and will not sensibly affect the results, in comparison with the inevitable errors of observation, especially if a, b, c are small. For a close circumpolar star, where $\sec \delta$ is very large and varies rapidly, it is advisable to make a special calculation.

In order to show how slightly precessional changes in declination, in a number of years, affect the values of the coefficients of a, b, c , I selected two stars, γ Ursæ Majoris and β Corvi, at the epochs 1840, 1880, both being near a maximum of precession in declination. The mean declinations for the beginnings of the years 1840, 1880 were taken respectively from the Greenwich "Twelve Year" catalogue and the Nautical Almanac. I calculated the level errors of γ Ursæ Majoris at the two epochs, and the azimuth errors of β Corvi, likewise at both epochs, assuming the very large azimuth and level errors of $3^{\circ}0$ each, or $45''$. I found in neither case is there an alteration amounting to $0^{\circ}03$, owing to the change of declination; obviously the change will be so much less if the instrumental errors are kept small, as they ought to be.

I have accordingly tabulated the logarithms of $\sin(\phi - \delta) \sec \delta$, $\cos(\phi - \delta) \sec \delta$, and $\sec \delta$, for all the Greenwich clock stars available in this latitude. The latitude and declination to the nearest minute (in my case the mean declination for 1880.00) and four places in the logarithms are amply sufficient. This is not a great labour, as there is no interpolation for seconds. The logarithms a, b, c only have to be added to obtain the logarithms of the results required. But presuming that no amateur would be content without taking the transits of at least five or six stars, the instrument being reversed for the last three, the logarithms of a, b, c can be written, *once for all*, on the lower edge of a slip of paper, neatly spaced out, and placed above the logarithms of the manuscript catalogue; and another piece of paper being held underneath, the sums of the logarithms may be written down, only involving writing the fifteen figures of the sums of the six logarithms. For the hint of this slip of paper, the keystone of this method, I am indebted to a kind friend, the able head of one of our public observatories.

An example will make this plain, thus: let the star be η Ursæ Majoris, and let the values of a, b, c be respectively $+0^{\circ}55$, $+0^{\circ}25$, and $+0^{\circ}067$, then we have the following calculation:—

(1)	9.7404	9.3979	8.8261
(2)	8.6139	10.1910	10.1912
(3)	8.3543	9.5889	9.0173
Numbers	$+0^{\circ}02$	$+0^{\circ}39$	$+0^{\circ}10$

The logarithms line (2) are those of $\sin(\phi - \delta) \sec \delta$, $\cos(\phi - \delta) \sec \delta$, and $\sec \delta$, respectively, as written in my MS. Catalogue against the star η Ursæ Majoris. The upper logarithms, line (1), are those of a, b, c , written on the slip of paper above mentioned,

and kept in its place by a finger of the left hand. Line (3), supposed to be written on the upper edge of another piece of paper placed under line (2), contains the sums of the three pairs of logarithms, the logarithms of the effects on the times of transit of the star, for the given values of a , b , c . The algebraical sum of the corresponding numbers will be the total effect of the instrumental errors*, thus:—

Azimuth	+0 ^o .02
Level	+0 ^o .39
Collimation	+0 ^o .10
Sum	+0 ^o .51

Let not an astronomical veteran (if such a one honour these lines with his perusal) carp at this process as *fiddling*, but rather let him try half a dozen examples, and, if I am not mistaken, he will be charmed with the facility with which it can be managed, and the results obtained in a very short time. A most useful adjunct to this method is a table of four-figure logarithms and anti-logarithms, thus described in the article "Table," by De Morgan, in the 'Penny Cyclopædia':—"1840. Anonymous (Taylor and Walton) Four-figure logarithms on a card. *Stereotyped*. Reprint of a table originally privately circulated amongst practical astronomers (see 'Companion to the Almanack' for 1841)." The Table of Anti-logarithms is reproduced in the British Association Star Catalogue, preface, pp. 90, 91. Those accustomed to turning over the pages of a book of logarithms will appreciate the advantage of finding everything they require, in these subsidiary calculations, at *one opening*. The late Augustus de Morgan, in the article "Slide-rule" in the 'Penny Cyclopædia,' thus characterizes this table:—"Between the sliding rule and the book of logarithms comes the card of four-figure logarithms." In the method explained above the table of anti-logarithms is very valuable.

Before concluding I wish to make one remark on the above example. It will be seen how small is the effect of the azimuth error on the time of transit of a star transiting near the zenith. I purposely selected this star for the sake of enforcing on the attention of the amateur the maxim, that if doubtful about his azimuth error, let him take his time from stars as near his zenith as possible. In one of our public observatories the time is obtained exclusively from zenith stars. The catalogue of 539 stars of the 'Astronomische Gesellschaft,' published annually at Berlin, is very convenient for this purpose; and in my MS. Catalogue of "Clock Stars" I have selected 42 zenith stars from this catalogue. By the way, is there not a misstatement in Loomis's 'Practical Astronomy,' p. 136? In reference to the practice at Greenwich, it is stated that "the error of the clock is determined exclusively by the

* Neglecting the difference in size of the pivots.

'Nautical Almanac' stars; and only those are used whose declination is less than 40 degrees." The reservation expressly excludes zenith stars.

Yours very truly,
GEORGE HUNT.

Hopfield, Alleyn Park, West Dulwich,
1882, November 3.

[No star, the declination of which exceeds 40 degrees, is observed at Greenwich for clock error; but since 1853 the stars chosen for this purpose have been by no means confined to those given in the 'Nautical Almanac.'—EDITOR.]

Effect of Irradiation with Varying Apertures.

SIR,—

In the last number of the 'Observatory' is a short note by Mr. Knott, giving the result of an attempt to determine the effect of irradiation on a bright disk, such as that presented by the image of the Moon. These observations have a particular interest for me, because, about ten years ago, I unsuccessfully attempted an enquiry, having the same object but by rather different means. Doubtless Mr. Knott had good reason for selecting the Moon as the object on which to conduct his investigations, though at first sight there appears some difficulty in the reduction of the observations, from the change of diameter with the altitude, the alteration of the position of the spot, &c., difficulties which are avoided by the selection of a more distant object. I confined my attention to the determination of the diameter of Jupiter and Saturn, where the effect of diffraction, whatever it be, is doubled, the aperture employed being about the same as in the example given by Mr. Knott.

Mr. Knott prefers to use a wire-micrometer for the purposes of measurement. In my unsuccessful attempt I employed both a wire-micrometer and a double-image micrometer, and it was owing to the impossibility of reconciling the observations made with these two instruments that I was forced to abandon the investigation. The only result to which I attained was that the peculiarities of observation induced by the employment of such dissimilar ocular arrangements introduced errors, or at least discrepancies, greater in amount than the quantity sought to be determined. I hope that Mr. Knott will prosecute his enquiry to a more successful issue.

The preliminary results, however, which that gentleman has submitted to the readers of the 'Observatory' are sufficiently interesting on general grounds. The discrepancy between the two results, viz. $54''.76$ and $56''.59$, surprises me, not by the smallness of its amount, but by its unusual magnitude. The observations that have hitherto been made to detect the effect of differences of aperture on the apparent diameter of an illuminated disk nowhere present such differences as those found by Mr. Knott. In 1853

MM. Liouville and Matthieu, at Paris, made a series of observations on the diameter of the Sun, by the method of transits, and found the following differences :—

Aperture.	Diameter.	Number of Observations.
152 mm.	D	24
71 „	D—0 ⁰ .01	40
35 „	D—0 ⁰ .01	14

(Comptes Rendus, vol. xxxviii. p. 253.)

In the following year, the same observers made a second series of observations on the Moon, which is the more interesting from being made in a precisely similar manner to that employed by Mr. Knott :—“ Je n'ai pas opéré sur le disque entier de la lune, mais j'ai mesuré la distance d'une tache à l'un des bords, avec et sans diaphragme.” The result was as follows :—

Aperture.	Diameter.	Number of Observations.
104 mm.	D	55
71 „	D—0 ⁰ .01	36
35 „	D+0 ⁰ .01	34

(Comptes Rendus, vol. xxxix. p. 372.)

Arago's observations are equally conclusive. He found that by restricting the aperture from 45 to 20 millim., the diameter of the bright ring around Arcturus was augmented 2'' in diameter, while the diameter of Venus, seen under the same optical arrangement, was diminished 0⁰.12. This result is confirmed by the observation of the Polar diameter of Jupiter. In a telescope, the diameter of whose object-glass was 104 millim., the diameter was found to be 37⁰⁰.08, and when the aperture was diminished to 29.5 millim. the diameter was 37⁰⁰.17. (Œuvres Complètes de Fr. Arago, vol. xi. p. 313.)

As Mr. Knott's observations differ so considerably from the results of other astronomers just quoted, it is to be hoped that he will continue his researches in this direction. The theory of diffraction, in the case of an illuminated disk, as investigated by Schwed* is not precisely the same that obtains in the case of a stellar image. Schwed has shown that the light gradually diminishes from the centre to the limb, and that at the real limb it has one

* Die Beugungserscheinungen aus den Fundamentalgesetzen der Undulationstheorie analytisch entwickelt.

half of the intensity that it had at the centre. Beyond the limb it rapidly diminishes, and at a distance of $4''.35$ from the true limb the intensity is only $\frac{1}{25}$ of that of the centre. At $5''.44$ the intensity is $\frac{1}{25}$. These numbers refer to a telescope with an aperture of one Paris inch, and vary inversely as the aperture of the telescope. They differ considerably from those that result from the consideration of the image of a star; and, I think, if Mr. Knott will compare his observations with the rigorous theory for an illuminated disk (if it be practicable), rather than that for a point, the agreement between observation and theory will be more conspicuous.

I am, Sir, yours truly,

W. E. PLUMMER.

Oxford University Observatory,
1883, Jan. 11.

Henry, Lord Clifford.

SIR,—

The reader of the 'Cycle' will remember Admiral Smyth's list of fifty English astronomical worthies between 1060 and 1599. To these we may add the name of one "Henry, Lord Clifford, who lived above twenty years disguised as a shepherd boy, until the accession of Henry VII. enabled him to assume his hereditary dignities with safety" (Rosa's *Summer Wanderings, Masters*, page 113), where it is also added, "He built Barden Tower, Yorkshire, for the 'study of astronomy, in which he did exceedingly delight'" (Pembroke, MS.). Do these Pembroke MSS. contain any further particulars of this worthy? He must have flourished at the time of the transits of Venus in 1518 and 1526. It is to be regretted that no ephemeris led him to look for the black speck on the Sun on those occasions. The only one in my possession for that time, Stöfler's, gives the conjunctions of the planet and Sun, but puts too wide a distance between them.

Faithfully yours,

S. J. JOHNSON.

Melplash Vicarage,
Bridport, Jan. 8.

The Transit of Venus.

SIR,—

The appearances presented by this celebrated phenomenon at various places apparently differed very much, and it seems important to collect every observation, in order, if possible, to elucidate further the causes of such differences. At Brighton we were fortunate in obtaining a view from the first contact to a few minutes before sunset almost entirely free from cloud, but the air was very unsteady. Owing to obstructions, I was obliged to use a 2.75 inch achromatic by Jones, with which the following notes were made. The power principally employed was, in conjunction with an erector, 74. The times are not reliable.

The first contact was not noted, as, owing to the great fluttering

of the Sun's limb, the planet had made a perceptible notch before it could be seen. At $2^h 7^m$ Venus was seen distinctly beyond the Sun's limb for a little way, and at $2^h 13^m$ the first signs of an atmosphere were seen, the limb of Venus off the Sun being edged by a light fringe. This fringe was very conspicuous on the southern portion of the limb of Venus, but very faint and narrow elsewhere. It was of a bright pearly lustre, quite different from the light of the Sun. The fringe of the light remained much brighter on the southern portion until $2^h 17^m 25^s$, when it was recorded as being nearly evenly bright, probably from the small portion of the planet then projecting beyond the Sun. The difference in the intensity of the light edging at different parts would no doubt be caused by the presence of clouds on the limb of Venus.

At $2^h 18^m$ the first signs of separation were seen, in the light fringe becoming alternately broader and narrower, no signs of a shade of any sort being then seen. At $2^h 19^m$, signs of separation had increased, and a shade was then visible filling the whole space between the limbs of Venus and the Sun for nearly the whole diameter of Venus; but this shade was so light and transparent that it did not interfere at all in obtaining distinctly a true geometrical contact, which was noted as occurring at $2^h 20^m 12^s$. No black drop was seen, nor any distortion of the planet, but the boiling was very great. I believe, however, that the time during which there was any doubt about the contact was very short indeed. After the contact, the dusky shade gradually contracted and became darker. At $2^h 21^m 15^s$ it was estimated at $\frac{1}{4}$ the diameter of Venus in breadth, and at $2^h 21^m 30^s$ the planet was quite distinct, but a narrow ligament occasionally just formed and connected the two limbs.

When on the Sun, the planet was examined with powers up to 306 with the erector, and a 75 astronomical eyepiece, but nothing was to be noted, no light specks of any kind being seen. With the red sunshade employed in all the previous observations, a light fringe was fancied, however, to surround the disk. Thinking it might be due to contrast, a much darker green glass was tried, and the fringe of light was then seen more clearly, and estimated at 5" or 6" (or possibly 10") in breadth. When the image was projected upon white paper and enlarged to reduce still further the contrast, this light border was also seen plainly.

The disk of Venus was considerably elongated at $3^h 34^m$, owing to refraction, and at $3^h 37^m$ a last glimpse was obtained as the Sun disappeared behind a bank of low driving clouds. It was remarkable how, when the Sun could only be faintly seen shining through the cloud, Venus came out as an intense, black and very prominent ball, and then seemed immensely nearer than the Sun.

Yours faithfully,

A. STANLEY WILLIAMS.

West Brighton,
1883, Jan. 15.

SIR,—

Having noticed in the daily journals that the “Transit of Venus” was well observed in Bath, and that a question was asked, at the Meeting of the R.A.S. held Dec. 8, whether any intelligence had been received from this district (*vide* ‘Observatory,’ Jan. 1883), I beg to send you, although somewhat late, my observations thereon.

I also enclose a slip from a local journal to which at the time I contributed a popular account of what I saw, but unfit for insertion in a scientific journal.

December 6, 1882.

1 P.M. Very clear sky overhead; cloudy towards horizon; wind N. About 1^h 45^m good transit observation “Vega;” noted sidereal clock error; resolved S.T. into G.M.T. and noted chronometer error. Sun quite clear of clouds; limb definition very unsteady; surface very good; group of small central spots; single spot near E. limb; reduced eyepiece from 150 to 100 and then to 60 to secure better definition of limb. 1^h 58^m. Slight banks of clouds rolling across Sun, making observation difficult. 2^h 1^m 40^s. Sun clear and detected Venus on edge; watched ingress for 7 minutes; edge of Venus sharp and well defined; no sign whatever of luminous margin, nor of white spot on disk. 2^h 9^m. Clouds came over and continued half an hour; afterwards clear till nearly sunset: Venus now well within Sun’s disk; examined her carefully again; no sign of margin; perfectly clean definition; intensely black; excessive distortion of disk, which increased as Sun approached horizon. Absence of luminous margin confirmed by my son, who was assisting me in noting time. This apparently agrees with Capt. Noble’s observation. Instrument 6-inch Equatoreal Refractor, Cooke & Sons.

Audley, Bath, 1883, Jan. 20.

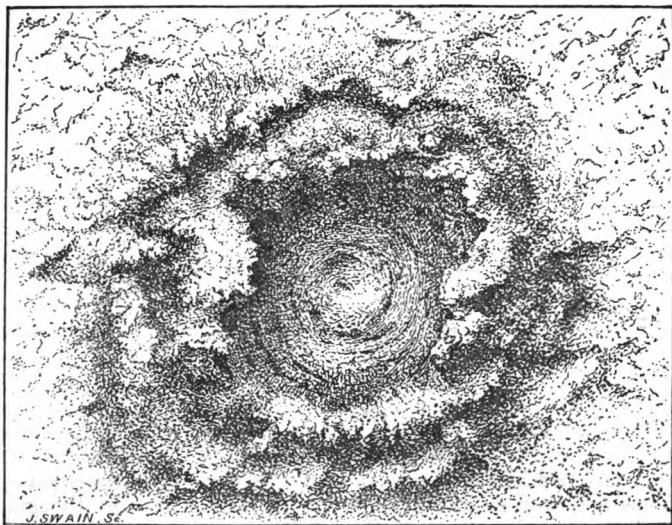
J. L. STOTHERT.

NOTES.

THE MODE OF FORMATION OF LUNAR CRATERS*.—M. Bergeron having noticed the manner in which gases or vapours, when they pass through a pasty mass, leave a series of funnel-shaped holes behind them, and struck with the analogy which these holes present to the craters of the Moon, has tried to reproduce the phenomenon on a larger scale, and for that purpose caused a current of hot air to pass through a mass of molten metal. For the convenience of the experiment the metal chosen was an alloy fusible at a comparatively low temperature, Wood’s alloy, which melts at about 70° C., being the first employed. A current of hot air was forced through the alloy, which was melted in a hot-water bath. Then, as the metal was allowed to cool slowly, the supply of air

* Comptes Rendus, Vol. xcv. No. 7.

being kept up, a bubbling was created, which drove away the particles which were beginning to solidify from over a considerable area, and a large ring was formed. The air still being blown



through, the edges of the ring rose little by little, and a perfect model of a crater was produced; and as the process of cooling went on a cone was formed, and the crater at the same time grew deeper, its inner slopes showing a much greater inclination than the outer. When the process of forcing the air through the alloy was interrupted, a second inner ring was formed, reminding the experimenter of the appearance presented by Copernicus, Archimedes, and other lunar craters. M. Bergeron considers that his experiments throw much light on the past history of the Moon. Instead of air, various vapours may have given rise to the craters and ring-mountains. These vapours rose freely from the Moon when it was in a fluid state, but the exterior of the planet being cooled much more rapidly than the interior, the latter, still fluid, continued to give off vapours when the surface had already become a pasty mass. These vapours passed through this envelope and found a vent at certain points only, doubtless where there was least tendency to solidification.

THE PARALLAX OF α LYRÆ AND 61 CYGNI, BY PROFESSOR ASAPH HALL (Washington, 1882).—Mr. Hall commenced his observations of the parallax of α Lyræ on 24 May 1880, and ended them on July 2, 1881, during which period measures were made on 77 nights. The method employed was that of differences of decli-

nation between α Lyræ and the well-known companion. The filar micrometer of the 26-inch refractor and an achromatic eyepiece giving a magnifying-power of 383 was used throughout. The resulting value of the parallax is

$$\pi = 0''.1797 \pm 0''.005612.$$

The extremely small probable error of this result is worthy of especial notice. Certain determinations of the parallax of α Lyræ have been previously made by other observers using the same comparison star. Brünnow in 1869, by measures of the distance and position, obtained the result

$$\pi = 0''.212 \pm 0''.0098.$$

Brünnow subsequently applied the same method as Hall used, viz. *diffs. of Decl.*, but to a different comparison star, $+38^\circ, 3241 (9.5)$, with results not very decided, but still showing a parallax between 0.131 and 0.199 . It seems impossible to doubt that the parallax of α Lyræ must be about one fifth of a second.

Prof. Hall has observed 61 (B) Cygni on 66 nights from October 24, 1880, to December 7, 1881. The comparison star is $+38^\circ, 4345$, the same as that used by O. Struve; but while Struve measured distances and positions, Hall measured differences of Declinations. Hall's result is

$$\pi = 0''.4783 \pm 0''.01381.$$

The mean difference of Declination between the two stars for 1881.0 is

$$\Delta\delta = -198''.513 \pm 0''.0112.$$

In the forthcoming part of the Dunsink observations will be found a determination, based upon observations extending over 12 years, of the difference of Declination between 61 (B) Cygni and the star $+38^\circ, 4351$, with the result

$$\pi = 0''.4756 \pm 0''.0321.$$

Astronomers in these climates will read with interest Professor Hall's remark that "During the spring months, when the observations came at an inconvenient hour of the night, General Hazen, of the Signal Office, very kindly sent me predictions of the weather, which proved to be remarkably correct." R. S. B.

THE LATE MR. C. V. WALKER.—We regret to have to announce the death, on December 24th last, of Charles Vincent Walker, in the 71st year of his age. Mr. Walker was well known as the Telegraph Engineer of the South-eastern Railway, a position which he had held since the first introduction of telegraphs on that railway, nearly forty year ago. He was one of those who very early saw the importance of using the telegraph for systematic train-signalling; and his name will also be remembered for the interest which he took and the valuable assistance which he gave in the establishment, in the year 1852, of the system of hourly time-signals originating at Greenwich Observatory, a system the first of the kind by which the electric telegraph became the means of dis-

tributing accurate time throughout the kingdom. Mr. Walker at one period paid considerable attention to the question of the earth-currents which appear in telegraph-wires, and contributed two papers to the 'Philosophical Transactions,' in which observations, made on wires of the South-eastern Railway system, when not employed on ordinary telegraphic work, are discussed, and the results compared with the magnetic changes recorded at Greenwich Observatory; and it was by his instrumentality that wires were subsequently erected on the South-eastern Railway lines solely for the continuous register of earth-currents, by means of photography, at Greenwich, from which valuable results have been derived. Mr. Walker took great interest in all matters relating to electricity, and was an active member of the old Electrical Society. He was afterwards elected a Fellow of the Royal and of various other scientific Societies, and served the office of President of the Society of Telegraph Engineers and of the Meteorological Society. W. E.

THE "EQUATOREAL COUDÉ" OF THE PARIS OBSERVATORY.—The Paris Observatory is at length in possession of this instrument, devised by M. Loewy so long ago as 1869, to overcome the drawbacks of the ordinary form of equatoreal, but the construction of which has been delayed by various untoward incidents to the present time. M. Loewy's experience in observing had made him acquainted with the inconveniences which attend the use of most equatoreals, the constant changes of the height and position of the observer's chair, the uncomfortable and awkward attitudes he is often compelled to assume, the constant interruptions occasioned by the need for turning the dome, which, with other lesser hindrances, all tend to fatigue and interrupt the astronomer, and at once render him unfit for delicate work and curtail the time, often precious in the extreme, at his disposal. These inconveniences are so serious, that, in certain observations, such as the search for comets, where a great extent of sky has to be examined, the observer is often compelled to use a small instrument on account of the fatigue involved in the use of a large one.

M. Loewy trusts that he has surmounted these difficulties by the equatoreal he has devised, and which MM. Henry, for the optical part, and MM. Eichens-Gauthier, for the instrument, have constructed under his instructions. The eyepiece and the observer's chair remain immovable, and the astronomer sits at his telescope just as he might at his desk. The equatoreal is bent at a right angle, as if it had been broken in the middle. The first part is parallel to the Earth's axis, and can turn on itself, and, as it revolves, the second part moves in the plane of the equator. At the angle where the two parts meet a mirror is placed at an angle of 45° to each, and a second mirror is placed before the object-glass, inclined likewise at an angle of 45° to its axis. The movements of the mirrors spare those of the observer, who can devote his undivided attention to the work in hand. The loss of light by the successive reflections

is but small. The outer mirror is 40 c.m. in diameter, the inner one much smaller. It is not feared that any distortion of the images will result from distortion of the mirrors, as these have been exceedingly carefully mounted. The observer and part of the apparatus are shut up in a little building. A light wooden hut shelters the rest of the instrument, and this is constructed to slide away on a little tramway so as to leave the telescope perfectly free whenever it is wished to observe with it.

SYDNEY DOUBLE-STAR RESULTS*.—An important contribution to our knowledge of the southern heavens has been made by Mr. Russell's publication, in a very compact little volume, of the measures of double stars made at the Sydney Observatory from 1871 to 1881. The catalogue comprises remeasures of about 746 of Herschel's stars and measures of 350 new doubles, the whole representing some 15,000 measures of angle and distance. Of the new pairs, nine are separated by less than one second of arc, and sixty-six by less than five seconds. The search for new pairs has, however, been merely incidental to Mr. Russell's main object—the reexamination of Sir John Herschel's Cape list between 34° south and the pole, a work the more important and valuable that no measures of any large numbers of these stars have been published since the appearance of that catalogue. Mr. Russell, however, remarks that though only an evening now and then was devoted to the search for new objects, the number recorded might easily have been doubled had he adopted the same limit of distance as Sir John Herschel. Only a few of the new stars have been repeatedly measured; but of these several show signs of motion.

Mr. Russell gives lists of objects in which his results differ from Sir John Herschel's. Thus in 46 cases he failed to find doubles where Herschel has recorded them, owing probably, in many cases, to errors in the Cape Catalogue, and in 17 other instances finds easy doubles in fields which Herschel examined without seeing any. Of these, one of the most striking is δ 4909, a group of five stars which Herschel described with great particularity, and which now shows a sixth within the pentagon formed by the others, and as bright as three of the exterior stars. Of stars which show real or supposed change since Herschel's observations, ρ or 6 Eridani seems, from the later measures, not to be a binary, as these observations plot into a straight line as if the preceding star had a separate proper motion. The doubles γ and π Lupi both seem to show motion; for whilst Herschel found γ easily separated and π excessively difficult, Mr. Russell has always failed to divide the former, whilst the latter is now an easy object.

The observations up to 1874 were made with a fine 7 $\frac{1}{2}$ -inch refractor by Merz, the powers ordinarily used being 159 and 330. Since

* 'Results of Double-Star Measures, made at the Sydney Observatory, New South Wales, 1871 to 1881,' under the direction of H. C. Russell, B.A., F.R.A.S., Government Astronomer for New South Wales.

then an 11½-inch refractor, of 12½ focus, by Schröder, has been used, with powers from 100 to 1500, 800 being employed for all difficult objects.

Mr. Russell indicates the date of the observations in an unusual manner, three columns being given with the "Day of the Month," "Month of the Year," and "Year in the 19th century," a decidedly less convenient method than the usual one of giving the year and fraction of a year.

E. W. M.

HARVARD COLLEGE OBSERVATORY*. — Some six years ago an effort was made to secure an annual subscription of 5000 dollars for this observatory. Prof. Pickering pointed out that a comparatively small sum, expended in increasing the efficiency of a well-equipped but insufficiently-manned observatory, would result in an amount of work out of all proportion to the sum expended. The money was raised; and Prof. Pickering now lays a report of the result before the generous friends who gave this timely help; and, without question, they must feel that the money has been most advantageously laid out, and that their experiment in the "endowment of research" has produced a result satisfactory beyond the most sanguine expectations.

Prof. Pickering thus summarizes the effect of the subscription:— "Without it, only one instrument, the meridian circle, was kept actively at work, the large telescope being comparatively idle. The reductions even of this one instrument could not be kept up, but every year fell more and more behindhand. With the subscription the large telescope, the meridian circle, and the meridian photometer are in constant use. A large number of the old observations have been published, while the remainder have been reduced, and before long will be ready for publication. One volume of the recent observations with the large telescope has already been published, another volume of meridian-photometer observations is now passing through the press. The unfinished volumes of 'Annals' were completed, so that our work is now known through twelve quarto volumes, while in 1876 but four had been given to the public. Eight more volumes of 'Annals' will be needed to complete the publication of the observations already made. The increased rate of work ensues simply because the corps of assistants has been more than doubled." The more detailed account which follows recounts the photometric examination of the satellites of Mars, and of the four major planets, of the eclipses of Jupiter's satellites, of more than two hundred double stars, of a hundred faint stars, of the planetary nebulae, of variables, especially of the Algol class, and of various standard points on the Moon. Then comes the search for new planetary nebulae, micrometric observations of the satellites of Mars, and the admirable arrangements made in connection with Mr. Ritchie of the 'Science Observer,' for the observation of newly discovered comets, computation of elements and ephemeris, and prompt dissemination of the information.

* Statement of Work done at the Harvard College Observatory, during the Years 1877-1882. By Edward C. Pickering, Director of the Observatory.

The various classes of observations made with the meridian circle, the work of the meridian photometer, Mr. Chandler's collection of all observations of variable stars, a long list of articles and papers on various astronomical subjects published by different members of the observatory during the last six years, with notes on a few observations of a more miscellaneous character undertaken during this period, complete this "Statement of Work," a statement which forms an eloquent tribute to the energy and skill of Prof. Pickering, and to the zeal with which he is supported by the other members of the Observatory, and which amply justifies him in coming forward now to ask for a permanent endowment sufficient to keep the institution, he conducts so well, in its present high state of efficiency.

The first part of Vol. xiii. of the 'Annals' of Harvard College Observatory, the volume referred to above, has recently been published. It contains the micrometric measurements made with the Equatoreal Telescope of 15 inches aperture during the years 1866-1881, under the direction of Profs. Winlock and Pickering. These comprise measures of double stars, of nebulae, of the satellites of Saturn, Uranus, Neptune, and Mars, of many of the asteroids, and of comets.

E. W. M.

THE TELLURIC LINES OF THE SOLAR SPECTRUM*.—M. Egoroff has continued his observations on the telluric lines by the comparison of the spectra from four sources of light placed at very different distances. An electric light, viewed through 10 kilometres of air, showed an immense number of lines in its spectrum. Four lines were seen between D and D₂. On the right and left of D, especially towards the red, they were very numerous and distinct. The group *a* was nearly complete. The region round C was also very rich. B showed its group partly resolved, followed by 11 pairs very regularly spaced out, and by the numerous lines which separate it from *a*. The two groups of *a* showed themselves resolved into very black lines. A was easily seen, not merely the group, but also the pairs which give it so close a likeness to B. Another light, viewed at 1600 m. distance, showed B, *a* and A very distinctly; two faint and nebulous lines between B and *a* were noticed, and some traces of absorption-lines near C and D. A distance of 240 m. showed only A, which was always distinct, and *a*, which was very faint, but which seemed somewhat intensified during a sharp shower of rain. A fourth lamp, only at 80 m. distance, showed no line but A, and this was hardly visible.

A NEW comet, near Jupiter, was discovered on Jan. 23 at the Puebla Observatory, Mexico. On Jan. 27 it was seen by an English observer about 1½° S.E. of Jupiter. It was then somewhat faint, about 4' in diameter, with but little central condensation.

WITH reference to the communication read by the Astronomer Royal at the November Meeting of the Royal Astronomical Society

* Comptes Rendus, Vol. xcv. No. 10.

(‘Observatory,’ No. 68, p. 358), the following eight observatories have fixed on Kiel as the centre for astronomical telegrams:—Copenhagen, Greenwich, Kiel, Milan, Paris, Pulkova, Utrecht, and Vienna. Each of these observatories will receive prompt intelligence by telegraph of every new astronomical discovery for an annual payment of £5.

THE death is announced, on Dec. 22 last, of Dr. Carl Hornstein. Dr. Hornstein was Director of the Observatory and Professor of Theoretical and Practical Astronomy at the Imperial Carl Ferdinands University of Prag, Corresponding Member of the K. Akad. der Wissenschaften in Wien, and of several other learned societies on the Continent.

Elements of the Orbit of the Great Comet of 1882.

By Dr. J. MORRISON, Nautical Almanac Office, Washington, D.C.

Observations.

Washington M.T.	<i>a.</i>	<i>δ.</i>	
1882. Sep. 19 ⁹⁶⁹⁷⁸⁶	168° 34' 44"·1	— 0° 34' 28"·5	Wash. Merid. Obs.
Oct. 8 ⁷²⁰⁴⁵⁷	157 1 37 '9	+10 40 21 '9.	„ Equat. Obs.
Dec. 11 ⁶¹⁹⁹²¹	124 5 25 '3	—30 16 28 '1.	„ Merid. Obs.

Elliptic Elements.

T.....	1882, Sept. 16 ⁹⁹⁸⁷⁶ , Washington M.T.	
Ω.....	346° 0' 19"·2	} Mean Equinox of 1882·0.
ω.....	69 39 31 '4	
i.....	38 3 3 '5	
e.....	·9998968	
log <i>q</i>	7·8900692.	
P.....	652·506 years.	

Motion Retrograde.

Middle Place.

C—O.

$\cos \beta \Delta \lambda = 0''\cdot60$

$\Delta \beta = 0''\cdot54$

The observations as given above were subsequently corrected for aberration and parallax by means of parabolic elements computed from the 1st and 2nd observations and from one taken at the Litchfield Observatory, Clinton, N.Y., on Dec. 24.

The residuals for middle place in the parabolic orbit were

$$\cos \beta \Delta \lambda = +23''\cdot0$$

$$\Delta \beta = -3''\cdot3$$

Ephemeris of the Great Comet, b 1882.*

By Dr. H. OPPENHEIM.

For Berlin Midnight.

	R.A.	Decl.	Log. <i>r</i>	Log. Δ .
1883.	<i>h m s</i>	<i>° ' "</i>		
Feb. 2....	6 8 44	— 21 39'5	0·4674	0·3591
4....	6 6 32	21 7'5		
6....	6 4 31	20 35'7	0·4757	0·3757
8....	6 2 40	20 4'2		
10....	6 1 0	— 19 33'1	0·4837	0·3923

* Dun-Echt Circular, No. 67.

Approximate Times of Transit of the great Red Spot and Equatorial White Spot across the Central Meridian of Jupiter in February 1883.

Date.	Red Spot on C.M.	White Spot on C.M.	Date	Red Spot on C.M.	White Spot on C.M.
	h	h		h	h
1.	13.4	12.7	16.	10.8	6.8
2.	9.3	8.4	17.	6.7	12.3
3.	5.2	13.9	18.	12.5	8.0
4.	10.9	9.6	19.	8.3	13.4
5.	6.8	5.3	20.	14.1	9.1
6.	12.5	10.7	21.	10.0	4.8
7.	8.4	6.4	22.	5.8	10.3
8.	4.3	11.9	23.	11.6	6.0
9.	10.1	7.6	24.	7.5	11.5
10.	5.9	13.1	25.	13.2	7.2
11.	11.7	8.7	26.	9.1	12.7
12.	7.6	14.3	27.	5.0	8.4
13.	13.3	9.9	28.	10.7	13.9
14.	9.2	5.6	Mar. 1.	6.6	9.6
15.	5.1	11.1	2.	12.4	5.3

An observation obtained at Bristol on January 18 showed the red spot to be central at about $11^h 57^m$ and the equatorial white spot at $14^h 23^m$. The times were rather uncertain owing to bad definition and the prevalence of clouds. Another observation on January 23 gave the following result:—Red spot central $11^h 1^m$; white spot central, $12^h 33^m$. Thus the red spot preceded the white spot $1^h 32^m$, so that the present conjunction of the two objects will probably occur on the evening of January 30.

Prof. Hough's observations of Jupiter, detailed in the last annual Report of the Dearborn Observatory, show that the principal white equatorial spot seen by him is identical with that which has been so frequently referred to by English observers during the last three years.

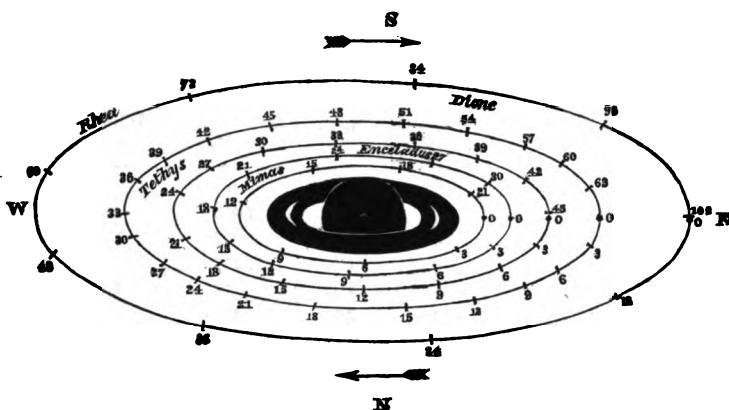
Between 1880 Oct. 28, and 1882 March 31, Prof. Hough determined the period of rotation as $9^h 50^m 9.8$ from 39 very exact observations. This is slightly greater than the period $9^h 50^m 7.42$ derived by Mr. Marth from 166 observations extending from 1880 Oct. 16 to 1882 Aug. 7 ('Monthly Notices,' vol. xlii. pp. 427-432).

The next conjunction of the white spot with the great red spot will probably occur on March 14.

Bristol, 1882, Jan. 25.

W. F. DENNING.

*Apparent Orbits of the Five Inner Satellites of Saturn as
seen in an Inverting Telescope.*



The arrows in the diagram show the direction of the motion of the satellites. The figures indicate the interval in hours from the time of last East elongation. The semi-major axis of the orbit of Rhea is $81''.23$ on Feb. 1, and $77''.41$ on Feb. 28.

The following table gives the Greenwich Mean Time of the East elongations of the satellites. In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS (East).		ENCELADUS.		TETHYS.		DIONE.	
	h		h		h		h
Feb.	3 11.9	Feb.	2 7.7	Feb.	1 21.5	Feb.	3 3.9
	4 10.5		3 16.6		3 18.8		5 21.5
	5 9.1		5 1.5		5 16.1		8 15.2
	6 7.8		6 10.4		7 13.5		11 9.0
	7 6.4		7 19.3		9 10.8		14 2.7
	20 11.1		9 4.1		11 8.1		16 20.4
	21 9.8		10 13.0		13 5.4		19 14.1
	22 8.4		11 21.9		15 2.8		22 7.8
	23 7.0		13 6.8		17 0.1		25 1.5
			14 15.7		18 21.4		27 19.3
			16 0.6		20 18.7		
			17 9.5		22 16.0		
			18 18.4		24 13.3		
			20 3.3		26 10.6		
			21 12.2		28 7.9		
			22 21.0				
			24 5.9				
			25 14.8				
			26 23.7				
			28 8.6				

MIMAS (West).		RHEA.	
	h		h
Feb.	12 10.8	Feb.	3 2.2
	13 9.4		7 14.6
	14 8.1		12 3.1
	15 6.7		16 15.5
	28 11.4		21 4.1
			25 16.4

Uranus may be found about 14° N and 11^{m} P of β Virginis.

Feb. 1, R.A. $11^{\text{h}} 34^{\text{m}} 9$, Dec. $3^{\circ} 35'$ N., tr. $14^{\text{h}} 47^{\text{m}}$
 28, 11 31 '2, 3 59 N., 12 57

Neptune is in quadrature with the Sun, Feb. 4, 16^{h} .

Feb. 1, R.A. $2^{\text{h}} 56^{\text{m}} 4$, Dec. $14^{\circ} 57'$ N., tr. $6^{\text{h}} 10^{\text{m}}$
 28, 2 57 '6, 15 4 N., 4 25

Phenomena.

G. M. T.			G. M. T.		
h	m		h	m	
Feb. 1	14 52	J. i. Oc. D.	Feb. 18	9 11	J. ii. Tr. E.
2	7	8 J. ii. Oc. D.	10	16	J. i. Tr. I.
	11 52	J. ii. Ec. R.	12	32	J. i. Tr. E.
	12	9 J. i. Tr. I.	19	5 30	B.A.C. 2872
	14 24	J. i. Tr. E.			Oc. D. 106° .
3	9 19	J. i. Oc. D.		7 28	J. i. Oc. D.
	12 35	J. i. Ec. R.	10	55	J. i. Ec. R.
4	6 36	J. i. Oc. D.	13	53	A ² Cancer
	8 51	J. i. Ec. R.			Oc. D. 95° .
5	7 4	J. i. Ec. R.	20	6 21	J. ii. Ec. R.
	10 58	J. iii. Tr. I.	7	0	J. i. Tr. I.
	13 39	J. iii. Tr. E.	9	6	ω Leonis
7	14 42	J. ii. Tr. I.			Oc. D. 56° .
9	7 52	J. iii. Ec. R.	23	8 8	J. iii. Oc. D.
	9 32	J. ii. Oc. D.	10	52	J. iii. Oc. R.
	13 58	J. i. Tr. I.	13	14	J. iii. Ec. D.
	14 28	J. ii. Ec. R.	14	27	J. ii. Oc. D.
10	11 9	J. i. Oc. D.	24	14 20	χ Virginis
	14 31	J. i. Ec. R.			Oc. D. 78° .
11	6 41	J. ii. Tr. E.	14	27	B.A.C. 4259
	8 26	J. i. Tr. I.			Oc. D. 74° .
	10 41	J. i. Tr. E.	15	40	χ Virginis
12	9 0	J. i. Ec. R.			Oc. R. 230° .
	14 36	J. iii. Tr. I.	15	48	B.A.C. 4259
15	10 5	B.A.C. 1563			Oc. R. 235° .
		Oc. D. 107° .	25	8 58	J. ii. Tr. I.
	16 5 59	χ^2 Orionis	11	44	J. ii. Tr. E.
		Oc. D. 139° .	12	8	J. i. Tr. I.
16	7 3	J. iii. Oc. R.	26	9 21	J. i. Oc. D.
	9 13	J. iii. Ec. D.	12	51	J. i. Ec. R.
	11 54	J. iii. Ec. R.	27	6 36	J. i. Tr. I.
	11 58	J. ii. Oc. D.		8 52	J. i. Tr. E.
17	13 0	J. i. Oc. D.		8 56	J. ii. Ec. R.
18	6 26	J. ii. Tr. I.	28	7 20	J. i. Ec. R.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image. EDITOR.

ERRATA IN No. 69.

Page 22, line 33, *for* Hooker *read* Hooke.

Page 23, line 1, *for* Kitchener *read* Kitchiner.

Publications received 1882, Oct.-1883, Feb.:—C. Leeson Prince, *The Illustrated Account given by Hevelius, in his 'Machina Celestis,' of the Method of Mounting his Telescopes and Erecting an Observatory, reprinted from an Original Copy, with some Remarks*—Edmund Ledger, *The Sun: its Planets and their Satellites* (Stanford, Charing Cross)—W. T. Lynn, *Third Edition of Main's Rudimentary Astronomy* (Crosby Lockwood & Co., London)—W. Harkness, *On the Transits of Venus* (Proc. American Assoc. for the Advancement of Science, Vol. xxxi. August 1882)—*Annals of the Astronomical Observatory of Harvard College*, Vol. xiii. Part I.: *Micrometric Measurements made with the Equatoreal Telescope of Fifteen Inches Aperture during the Years 1866-1881*—E. C. Pickering, *Statement of Work done at the Harvard College Observatory during the Years 1877-1882, and A Plan for securing Observations of Variable Stars* (University Press, Cambridge, Mass.)—*Instructions for Observing the Transit of Venus, Dec. 6, 1882*—Asaph Hall, *The Parallax of a Lyre and 61 Cygni* (Government Press, Washington)—J. P. Finlay, *Report of the Character of Six Hundred Tornadoes* (Professional Papers of the Signal Service, No. vii., United-States War Department)—Robert S. Ball, *Determination of the Annual Parallax of 6 Cygni B = Σ 2486* (Copernicus, Vol. ii.)—Schulhof et Bossert, *Ephéméride pour la recherche de la Comète 1812 (Pons)* (Mémoires de l'Observatoire de Paris)—Wilhelm Meyer, *Mémoire sur la Grande Comète Australe de 1880* (Mémoires de la Société de Physique et d'Histoire Naturelle de Genève, Tome xxviii. No. 1)—S. P. Langley, *Observations du spectre solaire* (Comptes Rendus, Vol. xcv. No. 11) and *Sunlight and Skylight at High Altitudes* (Amer. Journ. of Science, Vol. xxiv., Nov. 1882)—H. C. Russell, *Results of Double Star Measures, made at the Sydney Observatory, New South Wales, 1871-1881* (Government Press, Sydney)—Henry M. Paul and Prof. C. A. Young, *Longitude of the Observatory of the John C. Green School of Science, Princeton, N. J., and Edward S. Holden, Monograph of the Central Parts of the Nebula of Orion* (Government Press, Washington)—C. Piazzi Smyth, *Madeira Spectroscopic* (W. and A. K. Johnston, Edinburgh)—C. H. F. Peters, Nos. 1-20 of *Celestial Charts made at the Litchfield Observatory of Hamilton College, Clinton, N. Y.*—G. V. Schiaparelli and P. F. Denza, *Osservazioni delle Meteore Luminose nell' anno 1883*—*Minutes of Proceedings of the Intercolonial Meteorological Conference held at Melbourne, April 21-27, 1881* (Adelaide)—Latimer Clark, *Transit Tables*—Giuseppe Lorenzoni, *L'Astronomia in questi ultimi tempi* (Antonelli, Venezia)—*Osservazioni Astronomiche eseguite all' Osservatorio della R. Università di Padova, N. 2* (Antonelli, Venezia)—J. A. Westwood Oliver, *The Doomed Comet and the World's End* (Wyman, London)—*Tableau des Comètes apparues de 1871 à 1880* (Annuaire du Bureau des Longitudes, 1882)—J. M. Schaeberle, *A Method for Observing Artificial Transits* (Amer. Journ. of Science, vol. xxiii., Nov. 1882)—*Proceedings of the American Academy of Arts and Sciences, 1881-1882* (University Press, Boston)—E. Becker, *Logarithmisch-trigonometrisches Handbuch auf fünf decimalen* (Tauchnitz, Leipzig).

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No. 71.

MARCH 1.

1883.

ANNIVERSARY MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1883, February 9.

E. J. STONE, M.A., F.R.S., *President*, in the Chair.

Secretaries : J. W. L. GLAISHER, M.A., F.R.S., and
E. B. KNOBEL.

THE Minutes of the last Anniversary Meeting were read and confirmed.

The President. Before we proceed with the ordinary business, it is my painful duty to have to announce to the Society that Prof. Henry Smith died this morning at seven o'clock. I need hardly say that his removal from amongst us is greatly regretted ; indeed I am sure his death is a source of lasting sorrow to all who have had the pleasure of his personal acquaintance, and will be felt deeply by all who knew him as a public man and through his various works. (Hear, hear.) Undoubtedly, a great force for good has been taken from our small scientific world. (Hear, hear.) It is unnecessary for me to say more at the present time ; but I am requested by the Council to state that, on account of the vacancy created by Prof. Smith's removal, their wish is to substitute the name of Sir George Airy as Vice-President of the Society. (Applause.)

The Earl of Crawford. If I am in order, Mr. President, I would move that we express the deep sympathy of the Society with the surviving relative of Prof. Henry Smith, and that such expression of our feeling of sorrow and condolence be presented to Miss Smith, sister of the deceased.

Mr. Christie. I beg to second the motion.

The motion was unanimously passed, and the draughting of the resolution of sympathy was left in the hands of the President.

The report of the Auditors for the past year was read by *Mr.*
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Lecky, and was to the effect that having examined the Treasurer's account, and the account of the assets and property of the Society, they certified them to be correct; that the cash in hand on the 31st of December, 1881 was £549 10s. 7d.; that the funded property of the Society was the same as at the end of last year, and that, in addition, a sum of £500 had been placed on deposit account at the bankers'; that the books and instruments and other effects had been examined as far as possible, and found to be in a satisfactory condition; and that they had laid upon the table a list of the names of those Fellows who were in arrear for sums due at the last General Meeting.

The President. Unless some gentleman has any remark to make on the accounts, they will be laid on the table.

The Auditors' report was then laid on the table, without comment.

Mr. Glaisher gave a summary of the first part of the report of the Council for the past year. During the year 26 ordinary Fellows had been elected, and 13 had died; the total number of Fellows now being 598, an increase of 4. Five new Associates had been elected, and two had died; the total number of Associates now being 46.

The following are the names of the Fellows and Associates who have died during the year:—Rev. T. R. Robinson, A. Cooper, J. K. F. Zollner, H. Dodgson, E. H. Pringle, C. E. Burton, R. C. May, E. Plantamour, Rev. W. H. Drew, S. Heywood, Maj.-Gen. W. K. Worster, Rev. J. Challis, C. V. Walker, W. A. Cross, and T. Warner.

The usual reports had been received from a large number of observatories, and these were briefly summarized.

Mr. Knobel read the second part of the report consisting of the Notes on the Progress of Astronomy during the year. These were both numerous and important; the subjects to which they referred being as follows:—Small Displacements of the Plumb-line.—Investigations relating to the Tides.—The Micrometric Measures of the Harvard College Observatory.—Double Star Observations, several very important works having been published during the year.—Oppolzer's "Syzygien-Tafeln."—The Constant of Precession.—The Mass of Jupiter.—Discovery of Minor Planets in 1882, eleven having been discovered during the twelvemonth.—M. Gogou on a Lunar Inequality of Long Period due to the Action of Mars.—The Celestial Charts of Professor C. H. F. Peters.—Prof. Holden's Monograph of the Nebula of Orion.—The Harvard College Observatory Catalogue of Stars for 1875.—Dr. Huggins's Photographs of the Corona.—Astronomical Photography.—Houzeau's 'Bibliographie d'Astronomie.'—The Transit of Venus.—The Comets of 1882.—Prof. Langley's researches on the Solar Radiation.

The note on Dr. Huggins's remarkable discovery stated that in consequence of the photographs obtained during the eclipse of last May, which showed the coronal light to be strongest in the parts

of the spectrum between G and H, Dr. Huggins had been induced to endeavour to obtain an image of the corona by limiting the light to those particular rays. For this purpose he had employed both tinted glasses and coloured liquids. Between June and September twenty successful photographs were obtained, in every one of which the coronal form was exhibited with the characteristic rifts and rays. Different times of exposure were given to the plates, those exposed for the shortest time showing the inner corona only, whilst the plates exposed the longest showed the outer. There seemed little doubt but that both the inner and outer corona might be successfully photographed from day to day with a definiteness which would allow the changes always going on to be watched.

With regard to the Transit of Venus, 1882, *Mr. Knobel* said that details had not yet been received from all the different stations; but at the New Zealand station, Col. Tupman and Lieut. Coke had successfully observed the contacts, and similarly encouraging reports had been made with reference to the observations at Melbourne and Adelaide. At Brisbane, clouds prevented the observations, and their loss was much to be regretted, more especially as there were three observers at the station. At Sydney, Mr. Russell, Government Astronomer, and his staff had made elaborate arrangements for observing the Transit, but clouds prevented any thing being done. Capt. Wharton had observed both the egress and the ingress near Sandy Point in the Straits of Magellan—observations which would be of great value as a check on observations of the same phenomena. The German Expeditions had all been placed on the American Continent, and they were all partially successful in observing the contacts.

The President then delivered his address on the presentation of the Gold Medal awarded by the Council to Dr. B. A. Gould:—

GENTLEMEN,—Your Council have awarded the Gold Medal of the Society to Benjamin Apthorp Gould, for his '*Uranometria Argentina*.'

Dr. Gould is an astronomer who has held a leading position amongst the cultivators of our science for many years; and a reference to the "*Royal Society's Lists of Scientific Papers*," will find him credited with no less than fifty-five papers. These papers treat of almost all branches of our science; and some of them, such as the "*Reduction of D'Agelet's Observations*" are works of considerable extent and of great value. These works cannot have been without their influence in guiding the decision of your Council in the award of the Medal, but it has been upon Dr. Gould's direction of the work of the Cordoba Observatory that attention has been chiefly concentrated.

The astronomical results contained in the '*Uranometria Argentina*' constitute but a small and, in my opinion, not the most valuable portion of those which have been obtained by Dr. Gould since he assumed the direction of the Cordoba Observatory; but although the observations for the Southern Zones are understood to have

been completed, and one volume of results has already been published, your Council have thought it undesirable to await the complete publication of these results before marking, in the most emphatic manner in their power, their high appreciation of the skill and energy with which Dr. Gould has utilized the resources placed at his disposal by the liberality of the government of the Argentine Republic.

The work for which the Medal has been chiefly awarded may be considered an extension of Argelander's scale of magnitudes to all the stars which can be seen by a good eye without instrumental aid between 10° North Declination and the South Pole, together with a series of charts exhibiting, on a stereographic projection, the positions of all these stars to the sixth magnitude, and a proposed revision of the boundaries of the southern constellations.

The work is of a class which is not usually undertaken at our principal Observatories, for it requires neither an elaborate instrumental equipment nor that technical skill on the part of the observers which can only be acquired by long continued practice; but Dr. Gould, on his arrival, with four assistants, at Cordoba, in the September of 1876, found that the instruments which had been ordered from Europe for the Observatory had not been received; and the disorganization of scientific matters on the Continent, from the war then in progress, rendered it probable that there might be some considerable delay before these instruments could be delivered. Under these circumstances Dr. Gould, encouraged by the experience which he had gained, and by the success which had attended a somewhat similar effort, on a smaller scale, made during the mounting of the Transit Circle at the Albany Observatory in 1858, determined to undertake the work for which your Medal is this day awarded.

In the absence of any standard of absolute brightness, the determination of stellar magnitudes can only be relative, and the scale in which they are expressed must be an arbitrary one. The second Herschel, whilst at the Cape, determined the light received from the principal Southern stars in terms of the combined light of α^1 and α^2 *Centauri* as a unit, and he pointed out that by a slight modification of the scale in general use it would be possible to make that scale a photometric one, and the amount of light received inversely proportional to the square of the number which expressed the magnitude. But although some such modification as that proposed by Herschel would be a decided improvement, if the ground were not already preoccupied, yet our conventional system of magnitudes, as practically exhibited with some approach to scientific precision in the '*Uranometria Nova*,' has received such general acceptance from astronomers that probably Dr. Gould was well advised in adopting it for his Southern *Uranometria*. To secure consistency between the estimations of magnitude by the different observers and the practical adoption of Argelander's scale

and its extension to stars of the seventh magnitude, Dr. Gould selected, from the 'Uranometria Nova,' 1800 stars between the parallels of 5° and 15° North Declination which culminated at nearly the same altitudes at Bonn and at Cordoba. The magnitudes of these stars were estimated by all the four observers to whom the work at Cordoba was to be entrusted, and the results obtained were carefully compared with the magnitudes of the 'Uranometria Nova' and, I presume, in cases of discordances reconsidered. It was found that the estimations of the four observers were in perfect accord, to tenths of a magnitude, in the case of 722 of these stars; in the other cases the discordances did not usually exceed the tenth of a magnitude and never exceeded two tenths, except in cases where there were peculiarities of colour or the presence of some companion star to disturb the estimations. Dr. Gould determined to adopt as his standards of magnitude only those stars upon which the agreement of the four observers was perfect.

But to facilitate the comparisons of the Southern stars with these standards, stars in two regions south of Declination 55° were selected and carefully compared by all the four observers with the standards in the principal type-belt, and thus a secondary set of standards, more conveniently situated for direct comparison with the Southern stars, was established. The work was then distributed among the four observers, and appears to have been completed about the end of the year 1873.

It will give some indication of the magnitude of the work when I mention that the number of estimations made for the formation of the 'Uranometria Argentina' exceeded 46,000.

These estimations of stellar magnitude must be considered as the corpus of the work for which your Medal has been awarded; but Dr. Gould has carefully discussed the results, and compared them with nearly all the materials which were available for the purpose, and, in particular, he has compared his estimations of the magnitude of the brighter stars with results obtained from a discussion of the photometric observations of the second Herschel and of Seidel. The results of this comparison are generally satisfactory, although there appear some slight indications of a change of scale within the range of the four magnitudes compared with Herschel's measures.

The comparison of the Cordoba magnitudes with those of Lacaille, Herschel, and other astronomers has revealed some cases of marked discordance; but as Dr. Gould has had the great advantage of having his predecessors' work before him, and has been able to refer to the stars themselves for verification and, if need be, correction of his results, it is not probable that these discordances can be due to accidental errors in the Cordoba estimations. In the Notes appended to the Catalogue such cases are carefully pointed out, and discussed whenever sufficient materials existed for the purpose. These Notes will be of great value to our Southern

astronomers, and are certain to lead to successful investigation in the interesting field of stellar variation.

The maps published by Dr. Gould are fourteen in number, one of which is a skeleton map showing the proposed revision of the Southern boundaries of the constellations. Each map has been directly compared with the portion of the sky to which it refers before publication, and the greatest care has been taken to reproduce, as closely as possible, the gradations of light over the region of the Milky Way. The position of the Northern Pole of the Galactic Circle is fixed by Dr. Gould at R.A. $12^h 41^m 20^s$, Decl. North $27^\circ 21'$ for the epoch 1875.0. The maps are beautifully executed, and will well repay examination.

The materials collected by Dr. Gould on the Uranometria of the Southern heavens are far more complete and accurate than any which previously existed, and he has therefore been naturally led to discuss their bearing on those great questions of the constitution of our stellar universe which offer so fascinating and inexhaustible a field for philosophical speculation.

The results which Dr. Gould has obtained are in general accordance with those of previous investigators on the subject. It appears to be clearly proved that distance is one of the most important factors in producing differences of apparent brightness in the stars; but the agreement between the number of stars of different magnitudes and the number which might be expected if these changes of apparent brightness depended solely on distance is not perfect over any large range of magnitudes. There appears to be a decided preponderance in the number of the brighter stars. It is possible that this preponderance may be partially due to the conventional scale of magnitudes not being a truly photometric scale; and it is necessary, before any close agreement can be expected between the number of stars of the different magnitudes and those computed on the assumption of distance being the sole factor of variation, that the number of stars in a group should be sufficiently great to insure that the mean intrinsic brightness of the stars of each group should be constant, a condition which can only be secured when a considerable number of stars are included in each group, for there are certainly proved variations of intrinsic brightness amongst the stars in a higher proportion than 300 : 1. But, although Dr. Gould had these possible sources of error before his mind, he has been led, after a careful discussion of his own observations, to infer that the preponderance of the brighter stars is due to the existence of a stellar cluster consisting of some four or five hundred stars, of which our own system is supposed to be a member. The position of the northern pole of the medial plane of this belt of stars has been fixed by Dr. Gould at R.A. $11^h 25^m$, N.P.D. 60° , whilst that of the Galactic Circle is at R.A. $12^h 41^m$, N.P.D. $62^\circ 39'$. That there is a striking crowding of bright stars towards the portion of the heavens to which Dr. Gould calls attention is an undoubted fact, and one which had previously attracted

notice. It is also proved by Dr. Gould that the number of stars of the different magnitudes, which remain after the abstraction of the stars of this supposed group, agrees more closely than before with the computed number on the assumption that the changes in brightness depend solely on distance. But whether these facts are sufficient to prove our power of discriminating between the stars of this belt, and that general condensation of matter which gives rise to the appearance of the Milky Way is a point upon which I can form no very positive opinion. I think, however, that the differences between the annual parallaxes and apparent brightnesses of some of the stars of this belt show that, if it exists as an isolated cluster, it is one of vast extent and great complexity. To me, looking out from this mere speck of an Earth on the space which surrounds me, and recognizing, as I must, the awful and almost inconceivable distances that separate me from the nearest of the stars, compelled to acknowledge that every one of these twinkling points is as much an independent source of light and heat as our own Sun, that every increase of optical power brings a large proportionate number of other stars into view, and in entire ignorance of the position of my point of view amongst these stars, I feel my inability to firmly grasp the geometrical, much less the physical, constitution of such a universe. I cannot conceive space without limits, or the possible nature of a boundary of such space. However much, therefore, my mind may revel for a time and rejoice in its almost unfettered liberty amid such speculations, yet it finds no Mount Ararat upon which to rest the sole of its foot, and it soon gladly returns to the ark of experiment and observation upon which the foundations of our science have been laid, and the superstructure, if slowly, yet securely raised.

Although, therefore, from the constitution of my mind and training, I am perhaps unable to do full justice to this, the speculative part of Dr. Gould's work, I can speak with the greatest confidence of the value of the observational results contained in the '*Uranometria Argentina*.' It is a work of very considerable extent; it has been planned with great care, and executed with the most scrupulous attention to details. It will remain an enduring record of the relative brightness of the Southern stars for its epoch, and will be accepted for many years as the chief authority upon questions of their magnitude; and it is certain, from its very success, to incite other astronomers to efforts in the same direction—efforts which must ultimately lead to its being replaced by something more extensive, something even yet more accurate.

No higher praise can, I believe, be given to work of the class. In our science, at least, men do rise "on stepping-stones of their dead selves to higher things." I am certain that the award of this Medal by your Council to Dr. Gould will be received with the greatest satisfaction by the Fellows of this Society, and by practical astronomers of all nationalities, as a fitting tribute to good work done for the advancement of their science.

The President then handed the medal to the *Earl of Crawford* as Foreign Secretary, and said:—

MY LORD,—Will you kindly forward, through the representative of the Argentine Republic, this Gold Medal to Dr. B. A. Gould, with the assurance of our high appreciation of the skill, energy, and success with which he has directed the work of the Cordoba Observatory, and with the expression of our hopes that his efforts for the advancement of our science may be long continued and equally successful for the future. (Applause.)

The Earl of Crawford. I feel sure that I express the strong and sincere feeling of the Society when I say that our most cordial thanks are due to the President for his most interesting and able address. (Applause.)

The President. Will some Fellow please move that the Report of the Council be received?

Mr. De La Rue. I feel great pleasure in moving that the interesting Report of the Council, which we have heard read, be received and adopted, together with the Report of the Auditors, and that they may be printed and circulated in the usual manner, as well as the able Address of the President. I am sure it will be a great pleasure to read quietly and at our leisure the very eloquent and exhaustive Address of the President. (Applause.)

Mr. Rand Capron seconded the motion.

The resolution was then agreed to.

The President. Our next business is to appoint scrutineers. Three names have been submitted, which, I think, will meet with general approval, viz. Mr. Esdaile, Mr. Maunder, and Mr. Marth.

Mr. Esdaile declining to serve, Sir James Cockle was nominated in his place.

The President. Before we proceed to the ballot for the election of the Council, I may mention that the substitution of the name of Sir George Airy for that of the late Prof. H. Smith is the only alteration which the Council propose to make in the list. A question has been raised whether, in the case of an unaltered Council list being presented, the name of Sir G. Airy can be supposed to be substituted for that of Prof. Smith. (No, no.) It appears to me evident that the alteration must be actually made in writing. (Hear, hear.)

The ballot for the election of Officers and Council was then proceeded with, and resulted in the following list:—*President*: E. J. Stone. *Vice-Presidents*: Prof. J. C. Adams, Sir G. B. Airy, W. H. M. Christie, and J. R. Hind. *Treasurer*: Francis Barrow. *Secretaries*: J. W. L. Glaisher and E. B. Knobel. *Foreign Secretary*: William Huggins. *Council*: Captain W. de W. Abney, James Campbell, J. Rand Capron, Prof. Arthur Cayley, A. A. Common, G. H. Darwin, Warren De La Rue, A. M. W. Downing, Edwin Dunkin, George Knott, Rev. Charles Pritchard, and A. Cowper Ranyard.

The following gentlemen were elected Fellows of the Society:—A. W. Daughlish, Rev. E. F. Shaw, G. W. H. Maclear.

The Meeting adjourned at 4^h 55^m P.M.

A Plan for securing Observations of the Variable Stars.

[Continued from p. 51.]

THE problems to be undertaken may be defined as follows:—

1. To observe all the long-period variables once or twice every month throughout their variations according to such a system that all the observations may be reduced to the same absolute scale of magnitudes.

2. To observe the stars whose variability is suspected, and prove either that they are really variable, or that in all probability they do not belong to the first, second, or fourth class. If any are thought to belong to the fifth class, to watch them until such a variation is proved, or is shown to be improbable.

All of this work will depend on the possibility of readily determining the brightness of a star according to such a method that all the observations can ultimately be reduced to the same system. Herschel and Argelander have independently invented what appears to be the true method to be followed. If a star is seen to be very nearly equal to several others, from their light we can at any time define its brightness. It is essential that at least one of the stars selected should be a little brighter, another a little fainter, than the star to be observed. The range within which its light is known is thus also defined. Such observations will far exceed in value any direct estimate of magnitude. When stars are to be compared many times, it is convenient to designate them by letters for brevity. Let v represent a star which is suspected to be variable, and a an adjacent star of nearly equal brightness. Owing to fluctuations in the atmosphere, each star will appear to be constantly varying in brightness. If the stars appear equal after a careful examination, or if one appears brighter as often as it appears fainter than the other, we may denote this equality by av or va , these terms having precisely the same meaning. If one of the stars is suspected to be brighter,—that is, if it appears sometimes brighter and sometimes fainter, but more frequently brighter, the interval may be designated as one grade. The observation may be written $a\ 1\ v$ or $v\ 1\ a$, the brightest star being named first. If one star is certainly brighter than the other, the difference, however, being very small, so that they sometimes appear equal, the difference will be two grades, and may be written $a\ 2\ v$ or $v\ 2\ a$. Greater intervals may be estimated at three or four grades, but such observations have much less value. It is found in practice that a grade thus estimated will slightly exceed a tenth of a magnitude. A useful exercise for an observer is to select two stars of known magnitude and several others of intermediate brightness. Arrange them in a series in the order of brightness, and estimate the intervals in grades. The difference in magnitude of the first stars divided by the total number of grades gives the value of one grade. By using different intermediate stars, the same standard stars may be employed repeatedly. The following well-known

polar stars will be convenient, since they are always visible:—*a Ursæ Minoris*, 2.2 magn.; γ *Ursæ Minoris*, 3.0 magn.; δ *Ursæ Minoris*, 4.4 magn.; ϵ *Cephei*, 5.4 magn.; λ *Ursæ Minoris*, 6.5 magn. The above method is essentially that of Argelande. Sir William Herschel had already employed a method which differed mainly in his notation, α . . . , and — being equivalent to one, two, or three grades.

In all work of this kind the observer must look directly at the star he is observing at the moment, and never try to compare two stars by a simultaneous inspection of both. After examining one star until he has a distinct impression of its average brightness, freed from the momentary changes due to atmospheric disturbance, he should observe the other in the same manner. Alternate observations of the two stars, each observation lasting for a few seconds, will give a truer impression than can be derived from a simultaneous observation in which the two images must be differently placed on the retina.

The principal objection to this method is the difficulty of determining the value of a grade, as it is liable to vary with the observer, the time, the condition of the air, and the brightness of the stars. These difficulties are avoided by the following method. Select two stars for comparison; one, a , slightly brighter than the star to be measured, v , the other, b , slightly fainter. The interval between a and b should never exceed one magnitude. Estimate the brightness of v in tenths of the interval from a to b . Thus, if v is midway between a and b the interval will be five tenths, and we may write $a\ 5\ b$. If v is nearly as bright as a , we may have $a\ 1\ b$ or $a\ 2\ b$; if v is not much brighter than b , we may have $a\ 8\ b$ or $a\ 9\ b$. An advantage of this method is that larger intervals in brightness may be used between the comparison stars, and accordingly less distant stars employed. An increase in distance of the stars always renders the comparison more difficult. We can also obtain many independent comparisons by using several comparison stars. If we have m stars brighter and n fainter, we shall only have $m+n$ independent measures by the method of grades, while we have $m\ n$ comparisons by estimating tenths, since estimates may be made in terms of the intervals between each brighter and each fainter star. On the other hand, especially when observing stars not very near together, it is a decided advantage to have to compare two stars rather than three. Each method has its advantages, and that to be used should doubtless depend on the temperament of the observer.

Several precautions are needed to secure the best results. No observations should be made near the horizon; and, when the objects examined are at any considerable zenith-distance, stars differing several degrees in altitude should be avoided. If the stars are bright and there is no choice, a correction may be made for the error due to the varying absorption at these different altitudes if the time of observation has been noted. When using a telescope or opera-glass, the stars should be brought in turn to the

centre of the field, as when near the edge they will not appear of their true brightness. This is found to be better than placing them at equal distances from the centre. In selecting comparison stars, the proximity of a brighter star is very objectionable, causing a large error, which varies with the magnifying-power used. Double stars should be avoided if the power used is sufficient to show the companion. Comparing stars of different colours is also objectionable.

Any persons who desire to take part in these observations are requested to communicate with the writer, and send answers to the questions given below.

1. What is the location of your point of observation? In the city or in the country, on the ground, from a roof, or from a window? Is any part of your horizon obstructed, or can you observe in all parts of the sky?

2. What is the aperture, focal length, and name of maker of your telescope? also the lowest magnifying-power and largest field of view you can obtain with it? Have you a field-glass or opera-glass?

3. Can you identify bright and faint stars from their designations or right ascensions and declinations? Have you Heis' 'Atlas Cœlestis Novus,' the 'Uranometria Argentina,' the 'Durchmusterung,' or other maps and catalogues of the stars?

4. Would you prefer to observe the known or the suspected variables, or to divide your time between them?

For convenience in making the reductions and for future reference, it is essential that all the observations should be made according to the same system. Observers are accordingly requested to adopt the following form. Use half-sheets of letter-paper (eight inches by ten), writing only on one side and leaving a margin of half an inch for binding. Begin with a new sheet every evening, and write the date and location (township and state) on the first line. Each sheet when completed should be signed, and all should be numbered consecutively. When several sheets are used on the same night, the date should be entered on each. The record should be made in pencil, and all subsequent remarks or corrections added or interlined with ink, taking especial care not to obliterate or render illegible the original record.

A general statement should be made each evening of the condition of the sky, as "clear," "hazy," "passing clouds," &c. The time of beginning and ending work should also be noted. One line should be assigned to each comparison. The hour and minute should be written to the left, and the comparison next to it. The right-hand half of the line will be left blank for reducing the observation.

Certain evenings or portions of evenings must also be devoted to the selection of the comparison stars of suspected variables. If they are contained in maps which are available, the letters assigned to each star may be marked on the maps and lines drawn to show with what suspected variable star they are associated. If preferred,

a sketch may be made of the neighbouring stars and the letters entered on them. This sketch with a proper description should be entered on the observing sheets described above, and a copy should be retained for reference. Every month the observations will be interrupted by moonlight, and accordingly, three or four days before the full moon, all the sheets that have accumulated should be mailed, addressed Harvard College Observatory, Cambridge, Mass. An acknowledgment will be sent at once, so that if this is not received a second notification should be sent.

To attain success it is particularly important that the plan should not be local or national. Observers in the southern hemisphere are much needed, and for some purposes those in various longitudes. It is hoped that among the many amateurs of Europe, and especially of England, may be found some ready to participate in this work. No restriction regarding the observations or publication is intended; but it is hoped that a large addition to our present knowledge of the variable stars may be secured, without interfering with what would otherwise be obtained. Copies of this pamphlet and further information will be furnished on application. Any persons desiring to participate are requested to address the writer, sending answers to the questions given above. The details will differ with each observer, and will be arranged by correspondence. Apart from the value of the results attained, it is believed that many amateurs will find it a benefit to accustom themselves to work in a systematic manner, and that they will thus receive a training in their work not otherwise easily obtained outside of a large observatory. The lesson should be taught that time spent at a telescope is nearly wasted, unless results are secured worthy of publication and having a permanent value. Those who have once accomplished such work are likely in the future to appreciate its value, and will often continue to do useful work in some other department of practical astronomy, if not in that of variable stars. The education of a class of skilled observers would be a work of no less value than the results anticipated from the observation of the variable stars.

Harvard College Observatory,
Cambridge, Mass.

EDWARD C. PICKERING.

Arabic Names of the Stars.*

ALTHOUGH modern astronomers are disposed to set their faces against the retention of the old names for the individual stars which figure so abundantly in the earlier atlases and on our celestial globes, yet they are not without their interest and value. But few of

* The Names of the Stars and Constellations compiled from the Latin, Greek, and Arabic; with their derivations and meanings: together with the twenty-eight Moon-stations of the zodiac, known to the Arabs. By W. H. Higgins, M.B. (Copyright.) Leicester: Samuel Clarke, 5, Gallowtree Gate. London: Hamilton, Adams, and Co. 1882. 57 pp.

them can be expected to have any considerable lease of life in our catalogues and text-books, perhaps none besides those to which the 'Nautical Almanac' has offered an asylum, for a letter or a number affixed to a star serves all the purposes of modern astronomy as well and better. Still, though obsolete for all practical purposes, they will ever retain a certain archæological value; and an enquiry therefore into their etymology with a view to establishing their true orthography and meaning is not altogether devoid of interest, nor unworthy attention even in such practical days as our own.

The work before us is a neat little volume, written with just this purpose, to exhibit the meaning and correct the spelling of the old star-names. It is intended as the forerunner of a larger book which the author hopes to publish later on, upon the Names connected with the Stars and Constellations; but fearing lest he should find himself forestalled, and should so lose all credit for the arduous labours and research of many months, the author has hastily compiled the present work so as to establish his priority. The signs of haste are, however, limited to a few obvious mistakes, and the little book as a whole is well put together and clearly arranged.

The flavour of romance which seems to cling around names like *Betelgeux* and *Alderamin*, *Azelfafage* and *Denebola* somewhat evaporates when the words are explained. For the most part we find that the stars are named simply from their positions in the various constellations; thus *Betelgeux* is the Giant's, *i. e.* Orion's Shoulder; *Alderamin* the Right Arm, *i. e.* of Cepheus; *Denebola* the Lion's Tail. Prosaic as this explanation is, it gives the names a value they would not otherwise have, for we are enabled thus to trace out to some extent the limits of the original constellations and to perceive in what attitudes the figures attached to them were supposed to stand. Thus the name *Phact* of α Columbæ tells us that it was once the Thigh of Canis Major; *Alpheratz*, the name of α Andromedæ, shows that it has been stolen from the Horse, Pegasus; who has lost another star in π Cygni, *Azelfafage*, the Horse's Hoof.

Two stars, *Vega* and *Altair* (for *an nas'r al Waki* and *an nas'r at Tayr*), the Falling and Flying Eagles, are interesting as showing that they were bestowed in a much more southern latitude than ours, where *Vega* passed the meridian north of the zenith and hence seemed to be directing its flight from it instead of south of it as with us, with whom the constellation seems to be soaring upwards.

Some of the names, however, still remain unintelligible; probably their spelling has become so corrupted in the course of ages, that their true origin remains inexplicable. Thus it would seem strange if the Barker's Corner were the true rendering of *Zavigava*, β Virginis; nor is it clear why ϵ Canis Majoris should be called the Virgins, if that was the original meaning of *Adara*. Here and in one or two other instances Mr. Higgins distinctly improves upon Smyth and Ideler, for he suggests the etymology *ad dhahr*, the Back, of Canis Major that is, instead of *al ádhara*, the Virgins.

The mention of Admiral Smyth's name reminds us that Mr. Higgins would seem not to be acquainted with the 'Celestial Cycle,' as he says, p. 4; that "no English writer on Astronomy seems to have had any interest in tracing these 'Star Names' to their original language and thus assigning some meaning to them."

There is another English writer on Astronomy besides Admiral Smyth who has made a study of the star-names, but who unfortunately worked with far more zeal than discrimination. It is many years since "*Mazzaroth*" first appeared; but the strange and fanciful etymologies there put forth have lead many a reader astray, and at least one book published in the past year was founded entirely on Miss Rolleston's explanations, which have even found their way into official catalogues. Yet a very little consideration, even where all knowledge of Arabic or Hebrew was wanting, would suffice to suggest that Arabic names were more likely to find an explanation in the Arabic language than in supposititious Hebrew roots, and that meanings like that of Right Arm for *Alderamin*, the star in the right arm of Cepheus, or of Breast for *Schedar*, the star on the breast of Cassiopeia, were far more probable than Miss Rolleston's interpretations of "Coming quickly as in a circle," and "Freed," which have no obvious reference to the figures with which they are connected.

There are two cases, however, where Miss Rolleston has most manifestly tripped, and, oddly enough, Mr. Higgins also has made a slight slip in both instances. The two principal stars of *Libra* are generally known as *Zubenelgenubi* for α , and *Zubeneschemali* for β . These Miss Rolleston interprets as the "Price Deficient" and the "Price which Covers." Unfortunately, however, the scale marked with the stigma of deficiency, and which should therefore "kick the beam," is the one in which α shines, and is therefore really the southern and preponderating scale. The true explanation is that the names recall the time when the Scorpion held a double portion of the zodiac, and *Zubeneschemali* for β , the northern star, simply means the "Northern Claw," and *Zubenelgenubi* the "Southern Claw." Mr. Higgins, probably by a printer's error, gives Alpha's title to Beta, and *vice versa*.

The other instance is found in the names of α and β Delphini, *Svalocin* and *Rotanev*. Mr. Higgins's comment is, "The above words spelt backwards give the name Nicholas-Venator. From Niccolo Cacciatore—an Italian, who was a hunter." This is nearly, but not quite, the full solution of the riddle. The true history of their origin, first given, we believe, by Dr. Gould, is thus rendered by Flammarion in '*Les Etoiles*':—

"The search for the original Arabic of these names long baffled the utmost ingenuity of Admiral Smyth, as we can easily understand when we learn that they are only due to a somewhat childish joke of an astronomer in a jovial mood,—no doubt of Piazzi himself. These two names when reversed make, in effect, *Nicolaus Venator* ;

and the companion of Piazzi, at the Observatory of Palermo*, was none other than Niccolo Cacciatore, who died in 1841. Every one knows that *cacciatore* signifies hunter, in Latin *venator*. These two stars then simply bear the names of Nicolas Cacciatore, latinised and reversed!"

Miss Rolleston was not daunted by the uncouth aspect of these "cacophonous" words; and with a little alteration, no greater than many another star-name had to suffer at her hands, they slipped neatly into their proper places in her system, thus:—"Arab *Scalooin*, swift (as the flow of water);" and "Syr. and Chald., *Rotaneb*, or *Rotanev*, swiftly running (as water in the trough)." E. W. M.

*Account of Observations of the Lunar Crater Plato
in 1879-82.*

IN the years 1869-71, it will doubtless be remembered, the floor of the large walled plain Plato received a most searching examination at the hands of numerous observers; and the results of this examination, as collected and discussed by the late Mr. W. R. Birt, form a most important and interesting contribution to selenography, and are especially valuable from a selenological point of view. Mr. Birt once remarked, "it is hardly likely that an object such as the floor of *Plato* should remain in *exactly* the same state during a period of eight or ten years." With the exception of some observations by Mr. H. Pratt in 1873-74, the floor of Plato received little attention; but since March 1879 it has been subjected to another extensive examination by several observers; and a brief account of this I now propose to give, which, seeming strongly to confirm the opinion expressed in the above quotation, may therefore throw some light upon the subject of changes on the Moon's surface. At various times Mr. Birt published short notices of the progress of the examination; and it is greatly to be regretted that he was not permitted to discuss the results in an adequate manner.

Since March 1879 observations of Plato have been made by the following observers:—The Rev. F. B. Allison, Chesterfield (4½-in. Wray); W. F. Denning, Esq., Bristol (10¼-in. Browning reflector); T. P. Gray, Esq., Bedford (8-in. reflector and 8½-in. Calver); and by myself at Brighton (5¼-in. Calver and 2.75 in. achromatic); and to these gentlemen I am indebted for much kind assistance, and also to H. Pratt, Esq., of Brighton, who took an extensive part in the observations of 1869-71.

The chart of the interior of Plato has been prepared from the observations made between 1879, March 6, and 1882, March 6, chiefly from a large number of drawings and diagrams. The major axis and the positions of spots 1 and 4 are the same as those of the

* And in the preparation of Piazzi's Catalogue, in which the names first appear.

map on p. 247 of the 'British Association Report' for 1872; but, with these exceptions, the new chart has been prepared entirely without reference to the other, and it has been sought to give as accurately as possible the *usual* form and size of each feature. It will be more convenient to divide this article into three sections:—Section I. to be devoted to the consideration of the spots; Section II. to the streaks; and Section III. to the state of the floor and some other matters.

In his valuable "Account of Observations of the Lunar Crater Archimedes" ('Selenographical Journal,' No. 52, p. 33), Mr. Gray adopted the chronological method of discussion in preference to the arrangement according to the solar altitude, and, for similar reasons to those there stated, this method has likewise been adopted with Plato, but with the addition, whenever possible, of the Moon's age in days, and the character of the definition expressed according to the numerical scale of Mr. Birt*. It will no doubt be readily acknowledged that in 1869–71 the variations in form and brightness of the objects on the floor were, to a large extent, quite independent of the Sun's altitude, which is an additional reason for the adoption of the chronological method in the present case, especially as the additions above mentioned render it almost impossible that any well-marked variation due to the angle of illumination should escape being traced to that cause.

Section I. *The Spots.*

The spots shown in the map, and seen in 1879–82, number 38, as compared with 37 observed in 1869–71, but the spots were not the same in both cases; for whilst six spots seen in the former period were not seen in the latter, namely, 8, 10, 27, 28, 29, 35, seven *new* spots were detected during the recent observations, viz. 37, 38, 39, 40, 41, 42, and 43.

It will be a matter of some interest to compare the *mean visibilities* obtained for the two periods. The length of time over which the observations extended in each case is so great, and the observations themselves so numerous, that it is scarcely possible to imagine any serious error to affect the result; and the numbers may therefore be looked upon with great confidence as representing, in both cases, nearly exactly the actual mean visibilities or relative brightnesses of the spots. If we found that the visibilities for nearly every spot in one period were quite different from those in the other, our confidence in these results would be severely shaken; for, with our present knowledge of the permanency of the small detail of the Moon's surface, it would be most unnatural to find a large change in the brightness of nearly every spot in such a short space as ten years. This, however, is not the case in the present instance; for, with the exception of eight spots, in which evidently a very considerable change in brilliancy has taken place, the mean

* A short account of this scale will be found in the *Astron. Reg.* vol. xix. p. 123.

TABLE I.

No.	Mean visibilities in		Difference.	No.	Mean visibilities in		Difference.
	1869-71.	1879-82.			1869-71.	1879-82.	
0	·044	·031	- ·013	22	·170	·509	+ ·339
1	1·000	1·000	·000	23	·048	·013	- ·035
2	·039	·022	- ·017	24	·048	·261	+ ·213
3	·904	·865	- ·039	25	·144	·122	- ·022
4	·891	·977	+ ·086	26	·004	·013	+ ·009
5	·528	·540	+ ·012	27	·009		
6	·214	·297	+ ·083	28	·004		
7	·105	·113	+ ·008	29	·035		
8	·013			30	·166	·306	+ ·140
9	·218	·275	+ ·057	31	·026	·031	+ ·005
10	·057			32	·070	·077	+ ·007
11	·144	·022	- ·122	33	·013	·018	+ ·005
12	·026	·351	+ ·325	34	·022	·045	+ ·023
13	·148	·401	+ ·253	35	·004		
14	·432	·667	+ ·235	36	·004	·009	+ ·005
15	·017	·027	+ ·010	37		·004	
16	·293	·189	- ·104	38		·104	
17	·838	·784	- ·054	39		·068	
18	·083	·027	- ·056	40		·018	
19	·135	·162	+ ·027	41		·018	
20	·039	·027	- ·012	42		·022	
21	·022	·004	- ·018	43		·009	

4. Variable; usually fainter than 1, yet frequently equal to it, and more than once even surpassing it.

11. A large decrease in visibility. It was not seen by Mr. Pratt in 1873-74.

12. A very large increase in visibility, amounting to ·325. From a large number of comparisons of relative brightness, it was usually slightly fainter than 14, though a slight variation rendered it occasionally considerably fainter than, and sometimes equal to, No. 14. In common with 14 it generally had a hazy, ill-defined appearance. That its brightness is very different now from what it was in 1869-71 cannot be doubted, as in the present series it was seen 78 times, compared with *only six times* in the former period!

13. This also manifested a large increase in brightness, and exhibited curious changes in appearance. Very singularly, it was altogether missed in 1873-74 by Mr. Pratt.

14. The great rise in visibility of this spot in common with 12, and the striking similarity in the appearance of these two objects, would seem to imply an actual connection.

16. Besides the general decrease there were considerable variations in brightness, as in 1869-71.

22. Of all the spots this one evidenced the greatest change in brightness (.339), and must be considered as one of the most interesting of the spots. For two or three days after sunrise, on the spot, it usually had a very bright and very white appearance, which occasionally made it the most conspicuous object on the floor. It has been frequently seen as an elevated object; but, although examined on the most favourable occasions with powers up to 450, no signs of its being a crater-cone could be detected, although its sides were seen to be rugged. The spot varied much in brightness, and on two occasions, towards sunset, was the only object visible.

30. Generally a little fainter than 3, but, from a large number of comparisons, it varied between much fainter than, and equal to, No. 3.

34. A very peculiar spot; noted on one occasion as being "half as bright as 1."

38. From a large number of comparisons, 38 usually appeared slightly fainter than 5, but it varied slightly.

39. Generally as bright as 13, but more difficult, owing to the brightness of "Webb's elbow."

With regard to the new spots, all have not only been seen on at least two occasions, but also by two or more observers. On many occasions the spots visible were arranged in order of brightness; and the following sets will show how the order varied, irrespective of the Moon's age. This method, however, cannot be relied upon as giving very accurate results.

{ (8.6 d.)	1880, May 17.	1, 4, 3, 17.	Rev. F. B. Allison.
{ (8.8 d.)	March 19.	1, 17, 4, 3.	Rev. F. B. Allison.
{ (11.3 d.)	1879, May 2.	1, 17, 4, 3.	T. P. Gray, Esq.
{ (11.3 d.)	Dec. 24.	1, 4, 3, 17, 14.	Rev. F. B. Allison.
{ (13.2 d.)	1880, Sept. 17.	1, 4, 14, 22, 19, 3.	Rev. F. B. Allison.
{ (13.4 d.)	July 20.	1, 4, 3, 14.	T. P. Gray, Esq.
{ (13.9 d.)	Jan. 25.	1, 4, 22, 3.	A. S. Williams.

The complete absence of No. 17 in the third set is striking, although this spot was rated immediately after No. 1 in both the first and second sets.

Section II. *The Streaks.*

Table II. gives the mean visibilities of the streaks, derived from 714 observations, the sector being taken as the standard of comparison. These cannot be regarded as being quite satisfactory, but probably show pretty accurately the relative brightness of the different light streaks.

TABLE II.

Name.	Visibility.	Name.	Visibility.	Name.	Visibility.
<i>b</i> . . .	1'000	<i>a, o, κ.</i>	·679	<i>ν</i> . . .	·019
<i>c</i> . . .	·387	<i>β</i> . . .	·387	<i>a</i> ² ..	·047
<i>e</i> . . .	·472	<i>γ</i> . . .	·509	<i>b</i> ² ..	·028
<i>i</i> . . .	·500	<i>ε</i> . . .	·604	<i>c</i> ² ..	·009
<i>n</i> . . .	·151	<i>ζ</i> . . .	·641	<i>d</i> ² ..	·462
<i>p</i> . . .	·415	<i>η</i> . . .	·368	<i>g</i> ² ..	·057

The following is a brief account of the different streaks seen in 1879–82, comparing them with the same objects in 1869–71. The streaks *b* (the sector), *p*, and *ao*, particularly merit attention.

South-west Area.

The trident with its three arms ζ, ε, e.—The trident was only seen complete, without the stem *d*, on one occasion. More usually the whole of *ζ* and the portions of *ε* and *e* near the wall were visible; but *ζ* frequently stopped short of spot No. 1. As in 1869–71, there were variations in the relative brightness of the three arms.

*Streak g*².—This streak was discovered by Mr. Pratt on 1873 Nov. 1.

South Area.

Streak p.—One of the most interesting streaks on the floor, from the great probability of its being *new*. In the 'British Association Report' for 1872, pp. 249, 250, will be found the following passage:—"The observations had proceeded with great care during a period of more than twelve months when a new streak made its appearance between spots Nos. 5 and 14. Some months afterwards a continuation of this streak, eastward of No. 5, was observed, and very lately it has been seen between Nos. 14 and 22." Referring to his observations of 1873–74, Mr. Pratt remarked—"The streak from 22, through 14, 5, 17, to 6, has evidently brightened up during the last few months. If the whole had been visible some three years since, when separate portions of it were first seen, it is difficult to account for the additional portions not having been detected, *as the whole streak is now brighter than many other streaks previously recorded.*" The recent history of this streak is also very interesting. It was generally seen extending from spot 22, or 14, through 5 and 17 to 6, and apparently forming one streak with *γ*. It was usually broad, bright, and plain, the part of the sector crossed by it being *the brightest part of the floor*. On more than one occasion it was rated *fourth* in order of brightness, only the sector *ao* and *γ* being rated before it. The streak was also apparently variable in brightness. It scarcely seems possible that a streak repeatedly rated fourth in order of brightness could have

been overlooked for so long in 1869-71, and, when at length seen, seen in small portions only, if it had been as prominent then. Its apparent increase in breadth lately is also worthy of note.

A. STANLEY WILLIAMS.

[To be continued.]

The late Prof. Henry Smith.

On the very morning of the Anniversary Meeting, the Royal Astronomical Society lost one of its most gifted and distinguished members. The name of Prof. Henry Smith was one of which the Society might well be proud, for though not an astronomer in the strictest sense, his extraordinary attainments in mathematics, twin-sister to the science of astronomy, rendered his connection with the Society as natural as his high abilities and reputation made it honourable.

Henry John Stephen Smith was born in Dublin; but though an Irishman by birth, his life was passed almost entirely in England. As a boy he showed most remarkable precocity, and though only fourteen years of age when he entered Rugby, he was at once placed in the fifth form, the highest in which a new boy could be placed; nor was he long in securing his promotion to the sixth. On his going to Oxford he gained at the age of eighteen the Balliol Scholarship; and though obliged through illness to lay aside all study for many months, he yet succeeded in 1848 in winning the Ireland Scholarship, "for classical learning." Rather more than a year later he took his degree, obtaining a Double First Class in Classics and Mathematics, and in 1851 he gained the Senior Mathematical Scholarship, an honour, which, with this exception, has only once been obtained by an Ireland Scholar. His election to a Fellowship of Balliol followed in due course, and he held this Fellowship until a Fellowship free from the tutorial duties attached to the one at Balliol was offered him by Corpus Christi College. This offer he felt constrained to accept, as the demands upon his time were so numerous and pressing; but his connection with his old College was not thereby severed, as he was elected to an honorary fellowship.

Although he was a classical scholar of high rank, and though his acquaintance with the principal languages and literatures of Modern Europe was of a very intimate kind, it was as a mathematician that he was most distinguished, and mathematics was the science to the advancement of which he contributed most largely.

In 1861 he was elected to succeed the late Baden Powell as Savilian Professor of Geometry, a chair he held for twenty-one years; in the British Association he was President of the Mathematical and Physical Section in 1873; and he was President of the Mathematical Society from 1874 to 1876. Besides numerous scattered papers, contributed principally to foreign mathematical

societies, and of the merits of which the few qualified to judge speak most highly, he supplied in 1859 and following years to the British Association six most valuable reports on the Theory of Numbers, the results of his preliminary studies on a subject on which he bestowed much labour, and on which he was understood to have been preparing a great and important treatise.

But his mathematical reseaches formed but one part of his labours; he was constantly called to serve his University or his country in posts demanding in the highest degree practical wisdom and business ability and tact; and he ever fulfilled his duties with energy and success. He was one of the leading members of the Royal Commission on Scientific Education and also on that on the University of Oxford; and it was in connection with his nomination to serve on the latter body that Mr. Grant Duff remarked that "the Savilian Professor of Geometry is not merely in the first rank of European mathematicians, he would be a man of very extraordinary attainments even if you could abstract from him the whole of his mathematical attainments." "But his extraordinary attainments are the least of his recommendations for the office of Commissioner. His chief recommendations for that office are the solidity of his judgment, his great experience of Oxford business, his services on the Science Commission, and his conciliatory character, which has made him perhaps the only man in Oxford who is without an enemy." These qualities—his clear judgment, knowledge of affairs, and conciliatory character—rendered him a most invaluable, as he was a devoted, servant to his University, and well qualified him for the important and somewhat difficult post of Chairman of the Meteorological Council, to which the President and Council of the Royal Society unanimously nominated him; and in this position, so far as meteorology may be considered a branch of astronomy, he was able to do our science good service. As a Vice-President of the Royal Astronomical Society, and as a Member of the Board of Visitors for Greenwich Observatory, he was brought into closer connection with astronomy, but in neither position was there the need for him to display that activity and to exercise those powers that were called forth in other departments; and perhaps his essay on the controversy between Whewell and Brewster as to the "Plurality of Worlds" will have been the most direct contribution to the science which he made.

But though, as has been already stated, Prof. Smith was not strictly an astronomer, the Fellows of the Society and astronomers generally will feel a deep regret that one so gifted should be cut off at the comparatively early age of 56, and whilst still in the fullest possession of his rare endowments; and it will be generally recognized that in him we have lost, in our President's emphatic words, "a great force for good."

CORRESPONDENCE.

*To the Editor of 'The Observatory.'**Radiant-point of Meteors in the N.E. Region of Monoceros.*

SIR,—

On Nov. 7, 1869, Col. Tupman observed a sharply defined meteor-shower from the point $\alpha 124^\circ$, $\delta 4^\circ 5'$. It was also observed at Bristol on the mornings of Oct. 16–20, 1877, when the position was determined at $\alpha 121^\circ$, $\delta 0^\circ$, from the paths of six very swift meteors. The radiant-centre as derived from my observations is therefore $5\frac{1}{2}^\circ$ S.W. of that originally assigned by Col. Tupman; and these are the only references to this particular shower that I had met with, unless we assume that one of Schmidt's December radiants at $\alpha 120^\circ$, $\delta 10^\circ$, is really a prolongation of the same stream.

The recently published volume of '*Osservazioni di Stelle Cadenti*,' containing the apparent paths of 7602 meteors recorded by the Italian observers in the years 1868, 1869, and 1870, supplies an excellent confirmation to this hitherto scantily observed shower in Monoceros. On Oct. 22, 1870, $13^h 24^m$, Sig. Cobau at Girgenti observed a fine stationary meteor at the point $\alpha 123^\circ$, $\delta 1^\circ$. He notes that the duration was 2 seconds; it was first seen as a star of the 4th mag., but soon augmented to a brilliancy superior to that of Jupiter. This meteor obviously belonged to Col. Tupman's shower of Nov. 7, 1869, and with that observed at Bristol on Oct. 16–20, 1877. Comparing the three positions, we derive a mean of $\alpha 122^\circ.8$, $\delta 1^\circ.8$, which we may conclude is very near the radiant-point of this stream.

Yours faithfully,

Bristol, 1883, Feb. 2.

W. F. DENNING.

The Transit of Venus.

SIR,—

It may interest the readers of '*The Observatory*' to know that the Transit of Venus was observed with entire success at the Lick Observatory, on Mount Hamilton, California. In point of propitiousness of weather, we could scarcely have been more highly favoured. From midnight, November 30, until the afternoon of the day following the Transit, no cloud was in sight, day nor night, which could in any way interfere with any observation we had to make. During all this time the temperature was very mild, greatly facilitating the out-of-door work of mounting and adjusting the photographic instruments. The only untoward conditions of weather were the fitful gusts of wind which prevailed on December 3 and 4; these entirely died away during the morning of the 5th.

Our chief reliance in observing the Transit rested in the photographic method, the instrument employed being the horizontal photoheliograph. It was constructed in the main by Alvan Clark and Sons, and is essentially like the instruments made by the

same makers for the American Transit of Venus Commission. Its objective is five inches in diameter, its heliostat mirror seven, and its focal length very approximately forty feet, or $\frac{3}{4}$ th part greater than the mean focal length of the eight instruments of the Commission. The solar image on the plates is fully 4.5 inches in diameter. The wet process was adopted in taking the photographs.

The Sun rose about 7 o'clock, with Venus a good way on its disk. The first exposure giving a picture of satisfactory intensity was made at 19^h 17^m, local mean time, and shows the vertical diameter of the Sun something like a quarter of an inch shorter than the horizontal one. Valuable pictures were first obtained about half an hour later. The last photograph with Venus wholly on the Sun was taken at 23^h 38^m. After the third contact had been observed, a few additional exposures were made with the planet part way off the Sun's disk.

Nearly 150 exposures were made in all, and rejecting those nearest the time of sunrise, those between third and fourth contacts, a few worthless ones, and all others of doubtful value, the total number of plates which are available for micrometric measurement cannot fall far short of 125. The quality of these plates will not vary greatly, but about 75 of them are better than the remaining 50, and will admit of the highest degree of accuracy in measurement.

In addition to the photographic operations, the two contacts at egress were observed by Captain Floyd with the 12-inch equatoreal refractor of the Observatory, and by myself with the 4-inch transit instrument mounted on its reversing carriage.

Very faithfully yours,

DAVID P. TODD.

Lawrence Observatory, Amherst,
Massachusetts, 1883, Jan. 16.

NOTES.

THE GREAT COMET AND THE SPECTROSCOPIC METHOD FOR DETERMINING MOTIONS IN THE LINE OF SIGHT.—Amongst the many points of interest relating to the Great Comet of September last year, may be mentioned the beautiful example it has afforded of the method of determining the rate of motion in the line of sight of a celestial body, by means of the displacement of its spectral lines. Thus MM. Thollon and Gouy, observing the comet's spectrum on Sept. 18, remarked that the bright lines of sodium were displaced towards the red by $\frac{1}{4}$ or $\frac{1}{5}$ of the interval between the two D lines, corresponding to a motion of recession of 47 or 38 miles per second. The actual rate at which the comet was receding from us at the time of the observation, now that we know its orbit with sufficient accuracy to calculate it, proves to have been 45 miles per second, a sufficiently close accord, when it is remembered that the observation was made with a spectroscope of but small dispersion, and that the amount of the displacement was

only estimated, not measured. The observations made at Lord Crawford's Observatory, Dun Echt, corresponded closely in their result to those made by MM. Thollon and Gouy.

D'ARREST'S COMET*.—M. Leveau has recently presented to the French Academy of Sciences a note on the elements of this comet for its next return to perihelion. Owing to the comet not having been observed during its return in 1864, and to the great perturbations which it suffered through its near approach to Jupiter in 1861, he has not been able to comprehend in the same system of elements the observations made in 1851 and 1857 and those made in 1870 and 1877, and therefore has confined himself to elements representing those made during the two latter returns.

Adding to the osculating elements for 1877, Jan. 14^o, the perturbations produced by Jupiter, Saturn, and Mars from 1877 to 1883, M. Leveau obtains for 1883, June 12^o, the following osculating elements :—

Mean anomaly	328°	13'	20''34
Longitude of perihelion	319	11	10' 81
Longitude of ascending node.....	146	7	20' 98
Inclination	15	41	47' 11
Angle (sine=eccentricity).....	38	46	33' 42
Mean diurnal heliocentric motion....			53 ^o 65245
Mean Equinox 1880 ^o .			

From these elements it appears that the most favourable epoch for observation will be from 1883, April 23, to 1883, November 25. M. Leveau has calculated an ephemeris for this interval which he will publish in due course. It is evident, however, that only very powerful instruments can hope to detect the comet, since the relative brilliancy will range from 0.030 on April 23, to 0.091 on Nov. 25, whilst its value was 0.150 when M. Schmidt observed it last, with a refractor of 7 inches aperture, in August 1877.

THE TOTAL SOLAR ECLIPSE OF 1883, MAY 6.—Although the only stations from whence this eclipse can be satisfactorily observed are so distant, it has been felt that the results obtained during the brief totality of the eclipse of last May are so important that the opportunity afforded by the much longer duration of the forthcoming eclipse is not one to be neglected. Two English observers, Messrs. C. R. Woods and H. A. Lawrance, who acted as assistants to Dr. Schuster and Mr. Lockyer in Egypt last year, have therefore left England with the intention of joining the American Expedition at Panama, from whence they will proceed to Caroline Island near the Marquesas, where the total phase will last for nearly 5½ minutes,

* Comptes Rendus, Vol. xvi. No. 4.

A DOUBTFUL COMET.—There seems to be some reason to suspect that the new comet, the reported discovery of which at Puebla, was mentioned in the ‘Observatory’ for February, p. 65, was really Messier 1, the “Crab” Nebula. “Near Jupiter,” the only indication of the supposed comet’s place which the telegram supplied, is vague enough to allow of some uncertainty still remaining; but since no report of any further observations has reached us, and since several English astronomers have examined that part of the sky without finding any comet (the observer who believed he saw it on Jan. 27 having simply come across the “Crab”), it certainly looks as if a mistake had been made.

AMERICAN CENTRE FOR ASTRONOMICAL TELEGRAMS.—Prof. Spencer F. Baird, Secretary of the Smithsonian Institution, Washington D.C., has issued the following circular, under date Jan. 10, 1883:—

“Arrangements having been completed with the Director of the Harvard College Observatory for conducting the system of telegraphic announcements of astronomical discoveries, which was established by this Institution in 1873; you are hereby informed that from and after this date, the American centre of reception and distribution of such announcements will be ‘The Harvard College Observatory, Cambridge, Massachusetts,’ to which address all astronomical telegrams should in future be sent.”

DENNING’S COMET, 1881.—The last number of ‘Copernicus’ contains an interesting investigation by Mr. W. E. Plummer, of the University Observatory, Oxford, for the purpose of definitely determining the elements of the orbit of this comet. The comet itself is one of no ordinary interest. An addition to the number of short-period comets, it is remarkable as having a period intermediate between that of the group which recede about as far as the orbit of Jupiter and of Tuttle’s comet, which has its aphelion near that of Saturn. But the close approach which it makes to the orbits of Venus, the Earth, and Jupiter renders it more remarkable still. Mr. Plummer has computed an ephemeris for the comet during the period of observation from Dr. Hartwig’s second set of elliptic elements, and compared the observations with it. The effect of perturbations by each of the planets from Mercury to Saturn is next determined, and a corrected set of elements computed. The resulting osculating elements are:—

Epoch 1881, Sept. 28.5 Greenwich Mean Time.

Mean anomaly	1° 40' 35" 39	} Mean Eq. 1881.0.
Longitude of perihelion	18 36 12 78	
” ascending node ..	65 52 2 04	
Inclination	6 50 22 61	
Angle of eccentricity	56 8 28 42	
Log. semi-axis major	0.6315148	
Period of revolution	3235 days.	

CLARK'S TRANSIT TABLES, 1883*.—This very neat and handy little volume is especially designed for the benefit of those who, acting on the advice given by Mr. Latimer Clark in his interesting little work on the 'Transit Instrument,' have supplied themselves, or intend to do so, with one of the cheap and convenient little instruments he there recommends. For this purpose Mr. Clark gives a list, for every day in the year, of the mean times of transit of about twenty-four stars, together with their declinations and meridian altitudes at Greenwich. A large amount of more general astronomical information, such as the positions of the Sun, Moon, and Planets, the configuration of Jupiter's satellites, and the times of their eclipses, &c., is likewise added, and will render the book useful to the ordinary amateur.

DISCOVERY OF A NEW COMET.—A new comet was discovered by Messrs. Brooks and Swift on February 23. The following positions are given in two telegrams from Kiel to the Astronomer Royal:—

G.M.T.	R.A.	Dec.
1883, Feb. 23, 12 ^h 40 ^m 0	22 ^h 50 ^m 0	+28° 0' 0"
24, 11 58 3	23 7 23	+30 27 44

A NEW MINOR PLANET (No. 232), mag. 12.3, was discovered on Jan. 31, by Herr J. Palisa at Vienna.

Ephemeris of the Great Comet, b 1882.*

(Communicated by Vice-Admiral ROWAN, Superintendent
U.S. Naval Observatory.)

Computed from elements ('Nature,' 688), and reduced to the mean
Equinox 1883.0.

Greenwich Mean Noon.									
		R.A.			Decl.		Log. r.	Log. Δ.	
1883.		h	m	s	°	'	"		
Feb.	26....	5	52	14.7	—15	50	14	0.51133	0.45282
Mar.	2....	5	51	24.4	14	58	16	0.51841	0.46817
	6....	5	50	58.7	14	8	43	0.52532	0.48322
	10....	5	50	54.8	13	21	37	0.53200	0.49790
	14....	5	51	12.3	12	37	0	0.53861	0.51231
	18....	5	51	47.9	11	54	52	0.54508	0.52635
	22....	5	52	39.5	11	15	10	0.55135	0.53995
	26....	5	53	46.1	10	37	56	0.55751	0.55316
	30....	5	55	6.1	10	3	6	0.56354	0.56594
April	3....	5	56	38.1	9	30	34	0.56944	0.57828
	7....	5	58	20.9	9	0	19	0.57520	0.59015
	11....	6	0	13.9	8	32	21	0.58090	0.60158

Washington, 1883, Feb. 10.

E. FRISBY, Prof. Math., U.S.A.

Note.—In the published elements ϕ should be $89^{\circ} 13' 42''$ instead of $89^{\circ} 7' 42''$.

* Transit Tables for 1883. By Latimer Clark, Mem. Inst. C.E. Alfred J. Frost, 6 Westminster Chambers, London, S.W.

Approximate Times of Transit of the great Red Spot and Equatorial White Spot across the Central Meridian of Jupiter in March 1883.

Date.	Red Spot on C.M.	White Spot on C.M.	Date.	Red Spot on C.M.	White Spot on C.M.
	h	h		h	h
1.	6.6	9.6	17.	9.8	9.2
2.	12.4	5.3	18.	5.7	4.9
3.	8.3	10.7	19.	11.5	10.4
4.	14.1	6.4	20.	7.3	6.1
5.	9.9	11.9	21.	13.1	11.6
6.	5.8	7.6	22.	9.0	7.3
7.	11.5	13.1	23.	4.9	12.8
8.	7.4	8.7	24.	10.6	8.5
9.	13.2	14.3	25.	6.5	4.2
10.	9.1	9.9	26.	12.3	9.7
11.	4.9	5.6	27.	8.1	5.4
12.	10.7	11.1	28.	4.0	10.8
13.	6.5	6.8	29.	9.8	6.5
14.	12.3	12.3	30.	5.6	12.0
15.	8.2	8.0	31.	11.4	7.7
16.	4.1	15.5	Apr. 1.	7.3	3.4

On January 30 the white spot was observed by the writer as central at $11^h 44^m$, and the middle of the red spot followed at $11^h 49^m$, or 5^m later. The former was therefore 3^s in advance of the latter, and this shows the conjunction to have occurred at $2^h 47^m$ Jan. 30. The relative positions of the two spots on Jan. 30, as observed at Bristol, was exactly confirmed by Mr. A. S. Williams of Brighton, who noted their times of passage across the central meridian as follows:—White spot $11^h 47^m$, red spot $11^h 52^m$. Mr. Corder at Chelmsford also witnessed the transit of the white spot on this date; he estimated it exactly central at $11^h 50^m$. The following observations of this object were obtained in Feb. by the writer at Bristol:—Feb. 4 on C.M. $9^h 48^m$, 5th $5^h 24^m$, 7th $6^h 37^m$, 13th $10^h 9^m$, 16th $7^h 2^m$. Miss Ashley at Bath noted the spot central on Feb. 15 at $11^h 16^m.5$. This object has now again become rather small and faint, though it was especially large and brilliant between Jan. 23 and Feb. 7. It should come to conjunction with the red spot on about March 14, and it will be interesting to note the relative positions of the two objects on several nights at about this epoch.

The planet now sets so much earlier in the morning hours that transits of the spots are observable with much less frequency than hitherto. He continues, however, favourably visible during the more convenient part of the night, though his apparent diameter is becoming less, and hence the details of his surface are not to be distinguished with such facility as during the two preceding months.

W. F. DENNING.

Bristol, 1883, Feb. 21.

Astronomical Memoranda, 1883, March.

Sun. March 1, sets $5^h 37^m$, rises $18^h 46^m$; March 11, sets $5^h 55^m$, rises $18^h 23^m$; March 21, sets $6^h 12^m$, rises $18^h 1^m$; March 31, sets $6^h 30^m$, rises $17^h 38^m$.

Equation of Time:—Sun *after* Clock, March 1, $12^m 33^s$; March 11, $10^m 13^s$; March 21, $7^m 21^s$; March 31, $4^m 17^s$.

Sidereal Time at Mean Noon:—March 1, $22^h 35^m 8^s$; March 11, $23^h 15^m 2^s$; March 21, $23^h 54^m 6^s$; March 31, $0^h 34^m 1^s$.

<i>Moon. rises.</i>			<i>sets.</i>			<i>sets.</i>		
	h	m		h	m		h	m
Mar. 1..	13	44	Mar. 11..	9	30	Mar. 21..	17	7
2..	14	38	12..	10	49	22..	17	29
3..	15	26	13..	12	2	23..	<i>rises.</i>	
4..	16	7	14..	13	8	24..	7	29
5..	16	44	15..	14	4	25..	8	32
6..	17	16	16..	14	48	26..	9	35
7..	17	45	17..	15	26	27..	10	36
8..	<i>sets.</i>		18..	15	56	28..	11	36
9..	6	45	19..	16	23	29..	12	31
10..	8	8	20..	16	45	30..	13	19
						31..	14	2

Last Quarter, March 1, $17^h 26^m$; New Moon, March 8, $16^h 31^m$; First Quarter, March 15, $8^h 31^m$; Full Moon, March 23, $6^h 5^m$; Last Quarter, March 31, $8^h 21^m$.

The Moon is in conjunction with Venus, March 5, 10^h ; with Mercury, March 6, 22^h ; with Mars, March 7, 8^h ; with Saturn, March 12, 23^h ; and with Jupiter, March 15, 4^h .

Mercury is a morning star, at greatest elongation from the Sun, $27^\circ 13'$ W., March 3, 5^h , and at greatest distance March 11, 3^h . Mercury is in conjunction with δ Capricorni March 9, 6^h , and with Mars March 17, 5^h .

Venus is a morning star throughout the month, in her descending node, March 28, 11^h . She is in conjunction with δ Capricorni March 26, 13^h . Diameter:—March 1, $21''\cdot 1$; March 31, $16''\cdot 4$. Illuminated portion of disk, $0\cdot 625$ on March 15.

Mar. 1, B.A. $19^h 43^m\cdot 5$, Dec. $19^\circ 7'$ S., tr. $21^h 8^m$, rises $16^h 47^m$
31, 22 $0\cdot 6$, 12 21 S., 21 27 16 26

Jupiter is in quadrature with the Sun, March 12, 21^h . Diameter:—March 1, $37''\cdot 9$; March 31, $34''\cdot 5$.

Mar. 1, B.A. $5^h 23^m\cdot 9$, Dec. $23^\circ 2'$ N., tr. $6^h 47^m$, sets $15^h 0^m$
31, 5 35 $\cdot 6$, 23 15 N., 5 1 13 16

Saturn enters Taurus early in the month.

Mar. 1, B.A. $3^h 15^m\cdot 3$, Dec. $16^\circ 0'$ N., tr. $4^h 39^m$, sets $12^h 7^m$
31, 3 26 $\cdot 0$, 16 48 N., 2 52 10 25

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Feb. 28	39''·95	15''·51	26''·57	10''·32	15''·8
Mar. 20	38 ·75	15 ·33	25 ·77	10 ·19	15 ·5

The south side of the rings is visible, the elevation of the Earth above their plane being $22^{\circ} 51'$ S. on Feb. 28, and $23^{\circ} 18'$ S. on March 20, and of the Sun $24^{\circ} 3'$ S. and $24^{\circ} 12'$ S.

Uranus is in opposition to the Sun, March 11, 17^h.

Mar. 1, R.A. $11^{\text{h}} 31^{\text{m}} \cdot 1$, Dec. $4^{\circ} 0' \text{ N.}$, tr. $12^{\text{h}} 53^{\text{m}}$
 31, 11 26 ·4, 4 30 N., 10 50

Neptune is in Aries.

Mar. 1, R.A. $2^{\text{h}} 57^{\text{m}} \cdot 6$, Dec. $15^{\circ} 4' \text{ N.}$, tr. $4^{\text{h}} 21^{\text{m}}$
 31, 3 0 ·7, 15 19 N., 2 26

Phenomena.

G. M. T.			G. M. T.		
h	m		h	m	
Mar. 2	12 0	J. iii. Oc. D.	Mar. 21	9 35	J. i. Oc. D.
	4 11 33	J. ii. Tr. I.		22 8 54	J. ii. Tr. E.
	5 11 15	J. i. Oc. D.		9 5	J. i. Tr. E.
	6 8 30	J. i. Tr. I.		23 7 37	J. i. Ec. R.
	10 46	J. i. Tr. E.		24 8 3	J. iii. Ec. R.
	11 31	J. ii. Ec. R.		12 38 50	Virginis
	7 9 16	J. i. Ec. R.			Oc. D. 94° .
	12 6 51	o Arietis		13 51 50	Virginis
		Oc. D. 77° .			Oc. R. 217° .
	7 49	o Arietis		27 16 1 41	Libræ
		Oc. D. 304° .			Oc. D. 36° .
	13 10	J. i. Oc. D.		17 1 41	Libræ
	13 8 47	J. iii. Tr. E.			Oc. R. 305° .
	8 48	J. ii. Oc. D.		28 11 32	J. i. Oc. D.
	10 24	J. i. Tr. I.		12 0	ψ Ophiuchi
	12 40	J. i. Tr. E.			Oc. D. 119° .
	14 7 39	J. i. Oc. D.		12 54	ψ Ophiuchi
	11 12	J. i. Ec. R.			Oc. R. 217° .
	15 7 9	J. i. Tr. E.		29 8 45	J. i. Tr. I.
	20 10 3	J. iii. Tr. I.		8 49	J. ii. Tr. I.
	11 24	J. ii. Oc. D.		11 2	J. i. Tr. E.
	12 3 14	Sextantis		11 35	J. ii. Tr. E.
		Oc. D. 66° .		30 9 32	J. i. Ec. R.
	12 20	J. i. Tr. I.		14 44 16	Sagittarii
	13 17 14	Sextantis			Oc. D. 59° .
		Oc. R. 243° .		15 49 16	Sagittarii
	21 6 37 36	Sextantis			Oc. R. 303° .
		Oc. D. 70° .		31 8 35	J. ii. Ec. R.
	7 48 36	Sextantis		9 17	J. iii. Ec. D.
		Oc. R. 245° .		12 5	J. iii. Ec. R.

The angles are reckoned from the apparent N. point towards the right of the Moon's inverted image.

EDITOR.

JOH. AMBR. BARTH, éditeur,

LEIPZIG, Johannesgasse 34.

PERIODICA.

Annalen der Physik und Chemie. Herausgegeben von GILBERT (von 1799 bis 1824), von POGGENDORFF (von 1825 bis 1876), von G. WIEDEMANN (seit 1877). Jährlich 3 Bde. oder 12 Hefte. 8°. *M* 31. —

Beiblätter zu den Annalen der Physik und Chemie. Herausgegeben seit 1877 von G. u. E. WIEDEMANN. Jährlich 12 Hefte. 8°. *M* 16. —

Journal für praktische Chemie (von 1828 bis 1834 unter dem Titel: "Journal für technische und öconomische Chemie") herausgegeben von *Erdmann, Schweigger-Seidel, Marchand, Werther*; (bis 1869). Neue Folge (seit 1870) herausgegeben von H. KOLBE und E. v. MEYER. Jährlich 2 Bände in 22 Heften. 8°. *M* 22. —

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No. 72.

APRIL 2.

1883.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1883, March 9.

E. J. STONE, M.A., F.R.S., *President*, in the Chair.

Secretaries: J. W. L. GLAISHER, M.A., F.R.S., and
E. B. KNOBEL.

THE Minutes of the last Meeting were read and confirmed.

Mr. Knobel announced that 74 presents had been received since the last meeting of the Society. Amongst these was a beautiful photograph of the great nebula of Orion, taken by *Mr. Common*, who would probably give an account of it later on in the evening.

Mr. Downing read a paper on the relative motion of the components of ρ Eridani. *Mr. Russell*, the Government astronomer at the Sydney Observatory, in a recently published volume of double star observations, had stated his opinion that all the more recent measures showed that the motion of the companion star was in a right line, and not in an elliptic orbit as had hitherto been supposed. *Mr. Russell* rejected altogether the observations made by *Dunlop* in 1825, and *Herschel's* measures of distance made in 1835, retaining, however, his determination of position-angle. *Dunlop's* observation was undoubtedly discordant from the later ones; but after rejecting this one, *Mr. Downing* found that the observations subsequent to *Herschel's* accorded somewhat better with the hypothesis of an orbital motion than of motion in a right line—the sum of the squares of the residuals being nearly the same in each case in position angle, and smaller for the ellipse in distance; the orbit he had deduced giving much the same eccentricity as that which *Dr. Doberck* had computed, but a longer period, 224 years instead of 117 years. He had also

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looked at the proper motions of the two stars, and it appeared to him that, so far as the observations went at present, they rather confirmed the probability of elliptical motion; so that on the whole it seemed more probable that the pair formed a true binary system.

Dr. Doberck thought that the matter was still very uncertain. It was very often found necessary to change the elements of a binary system very much after a few years' interval, so that his own investigations could not be regarded as in any way settling the question. He was exceedingly glad that *Mr. Downing* had taken the subject up. It would depend, however, principally on observations that might be made in the future, whether it was decided that the companion moved in a straight line or in an elliptical orbit.

The President. We have the pleasure of seeing *Dr. Gould* here to night, and I am sure it will be a great satisfaction to the Fellows to hear any remarks he may be pleased to make in reference to the work on which he has been engaged at Cordoba. (Applause.)

Dr. Gould. *Mr. President*, the humblest clergyman is not expected to deliver his discourse until he has a text; and I have not prepared myself with any; but out of the abundance of the heart the mouth speaketh; and if there is any point of our work at Cordoba which may be of interest to the Society, it will gratify me very much to answer any questions. But first of all, before I say any thing on that subject I have to express, in the most earnest manner, my sincere and hearty thanks to the Royal Astronomical Society, to its Council, and to yourself, *Mr. President*, for the very great and unmerited honour which they have recently paid me. I arrived in England on Saturday last, and the welcome which awaited me was most unexpected, and I was quite overcome by it. A friend suggested to me a day or two ago that as there would be this meeting of the Society this evening, and as I should unquestionably desire to be present, it might be worth while to give some little statement with regard to one of the principal features of the work at Cordoba in which I have specially interested myself—that is, the photographic work. Coming here to speak on astronomical photography in a Society where *Dr. De la Rue* and *Mr. Common* have given such demonstrations of what they can do, is a somewhat bold thing to do; but I have here two or three photographs of clusters of the Southern Hemisphere which will give you a general idea of what we have done at Cordoba, and I think they give some promise of future usefulness. As you will remember, *Sir*, you and I started on our respective expeditions to the southern hemisphere in the same week, though I have not had the good fortune to finish my work as soon as yourself. I told you then that I hoped to do something in the line of stellar photography.

During the course of the various examinations which I have had occasion to make of the southern heavens, I was able to select some 40 or 50 clusters which seemed to give great promise, and of these

I have a number of photographs. They were taken on a small scale on glass; and my friend Mr. Rutherford has kindly given me a beautiful and excellent micrometer, with which I hope to be able to carry out the measurements. I have no reasonable ground for expectation that I shall be able to begin immediately; but there are, beside these clusters, four stars which we suspect to have an appreciable annual parallax. Three of these scarcely pass below the horizon, and the fourth is visible for three quarters of the year. Before I left Cordoba, I had a few of these groups enlarged on paper, in order that I might insert the names of the stars; and I have three or four of them here. One is κ Crucis, a beautiful cluster popularly supposed to present all the colours of the rainbow. The second is α Carinæ; two others are the nebulae in Scorpio, 6 and 7 Messier. The grand point in these photographs is to get the exposure long enough to obtain the faint stars, and I think we have been successful in getting them down to the $10\frac{1}{2}$ or 11th magnitude. (Hear, hear.) That of course requires a very long exposure, and the great point is to have the dot round. The great trouble is in the clock-work,—to secure that the motion of the equatoreal shall be absolutely uniform during a sufficient period; because, though the mean rate of the clock may be correct, yet if the actual rate vary during the time of exposure, the impression will be a line and not a dot. Dr. Gill writes to me from the Cape that he has measured one of these parallax stars at the Cape with the heliometer, and that in his series of observations he has found no essential parallax. That is not a very encouraging prospect for our measurements. Amongst other stars of which I have taken pains to secure photographs, are ζ Hydri, ϵ Indi, and the very notable star β Hydri. There is also a star, 9352 in the Lacaille Catalogue, with a remarkable proper motion, almost as large as that of Groombridge, 1830. I have a micrometer, but I have nothing else, and do not know how many years will elapse before it is possible to submit these photographs to measurement. I have been very unfortunate at first, but not in the long run, with my photographers. The first gave me a great deal of trouble, and his photographs failed in a great measure, so that I had infinite uneasiness in regard to the results. The second did excellently, but his health failed him. The work was then dropped for 2 or 3 years. The last time I was here, I engaged the services of Mr. Thompson, a very excellent and competent man; but his health also gave way; but, fortunately, before he left, one of the astronomical assistants (Mr. Stevens), being a person of versatile character, became very much interested in the work, and Mr. Thompson gave him the necessary instruction, so that with his help the entire list of the star clusters has been completed. You, Sir, have brought back the stars on paper, and I have brought them on glass.

The President. I shall be glad to know when there is a probability of your zone work being completed and published.

Mr. Gould. The Zone catalogue afforded the happy occasion of my now coming to London. The observations of the zones were finished in 1875; but the reductions have given, naturally, a very great deal of trouble, because, my staff being limited, their time was occupied with observations and their immediate reductions, and this mass of 105,000 observations of zones they had no time for. I have been obliged to enlist into the service everybody I could find who had had sufficient education to be of use. I have had bakers, shoemakers, printers, carpenters, bricklayers and schoolmasters, sailors and engine-drivers. Of course the degree of accuracy was sometimes questionable, and I have been obliged to do every thing in duplicate. That entailed the necessity of a comparison, and that implied reliance upon their uprightness and honesty and so on. But when the errors were once discovered and corrected the work turned out fairly well, subject to some little criticism. Now the work has been done in duplicate and compared, we find there are about 75,000 different stars, the remainder being duplicate and triplicate and sometimes quadruplicate observations of the same star. Where any of these stars had been already observed by Lacaille, Brisbane, Taylor, Gilliss, Moesta, or Ellery, or at the Cape (I was not fortunate enough to have your catalogue, Mr. President) their places were roughly brought up to the adopted epoch, 1875; and thus if there were any gross errors (and we found some) they were corrected, the star in some cases being reobserved. But last October I was forced to consider the case of possible errors in those stars which had not been observed in duplicate by ourselves or other astronomers. There were about 30,000 observations, which, being single ones, represented 30,000 different stars, of which we could find no other observations, and there seemed to be a considerable danger of errors among them. After so many sacrifices as I had made for the sake of these zones, it seemed undesirable that any errors which could be discovered should be allowed to go undetected; and I left my very excellent and trusty corps of assistants—four young men out there on whom I can depend for any thing and every thing—to make a revision of these stars which have not been otherwise observed. The stars are not fully reobserved; but the observers look strictly to see whether each one passes the middle of the field at the right time and at the right setting in declination. I do not know that I can tell you what is the proportion of errors; but we have found a considerable number. We began upon the zone catalogue which had been previously supposed complete, but which is now subject to this new revision, at the 23rd hour. At the time I left Cordoba we were at work at the 8th hour, hour 6 having been completed. In my absence the work is going on, I have no doubt, with the same zeal and accuracy as if I were still there; and I have reason to believe the entire revision will be completed by the beginning of July. Meantime the “copy” is to be sent on for revision, and I have brought with me the first six hours, which I purpose giving at once to the printer that they may be

stereotyped, while the ordinary star observations of 1873, 1874, and 1875, in which year the zones were ended, are being printed in Buenos Aires. I trust that in less than a year from this day, if health and strength and funds are spared me, the zone catalogue, in 2 volumes, with 74,600 stars, will be ready to lay upon the table.

In the meantime another work has been going forward.

In observing for the zone catalogue, the stars have been taken only across a single group of five threads in right ascension, and their declinations have been determined from the reading of a single microscope, all four microscopes being read at the beginning and end of the zone in order to determine the deviation of the single microscope No. 1 from the mean of the four. At the same time, however, other observations have been made for a general catalogue. In this work each star has been observed upon three groups or 11 threads in right ascension, and the declinations determined by all four microscopes, and the nadir point, level, and collimation determined three times each evening, so that I think the observations may claim to be as accurate as instrument and observer can make them. That general catalogue contains about 121,000 observations, and I do not know how many stars; but if the number of different stars in each year be added, it would make 41,500. A great many stars were of course observed in two or more years, so that I hardly venture to hope that there will be more than 33,000 or 34,000 different stars in the catalogue. This catalogue differs from the zone catalogue in several respects, and especially in the greater accuracy with which the observations have been made. This catalogue also includes every part of the southern hemisphere, and extends from the equator to the pole, whereas the zones extend only from the tropic to within ten degrees of the pole. I have no doubt there are many stars even as bright as the 8th magnitude which have escaped us; but there are a great many fainter than the 8th magnitude which have not escaped. If a star from the 8th to the $8\frac{1}{2}$ magnitude is needed for any purpose, the chances are they will be found in this catalogue; and as, if they are not in the Cordoba catalogue they will be nearly certain to be found in the Cape catalogue, I think the southern hemisphere may be considered as safe for most of the stars brighter than the 9th magnitude. In preparing this catalogue the stars have already been reduced to the mean equinox of the beginning of the year of observation, and the precessions and secular variations have been computed in duplicate for all, compared, verified, and corrected. I shall be very much disappointed if 18 months more does not see this catalogue finished; and then I shall feel I have a right to come home. (Loud applause.)

The President. I am sure we have all listened with very great pleasure and satisfaction to Dr. Gould; for my own part, I must say with exceedingly great satisfaction. Curiously enough, Dr. Gould started for Cordoba and I for the Cape within the same

week. We happened to meet each other just before we left England, at the dinner of the Astronomical Club, and we both were determined to carry out our work. I finished my catalogue and came home. Dr. Gould has been able to do a good deal of miscellaneous work that I was not able to touch, as I had a great incubus of work left upon me that I was bound in honour to take up. Independently of that, however, I set to work making my catalogue; and having finished it, I came home satisfied. I only hope that when Dr. Gould has done his work he will come home equally satisfied, with a yet greater work, and it looks to me very likely that he will do so. With regard to these photographs of the clusters, they are exceedingly interesting, and they certainly indicate that photography is coming very much to the front in all cases where absolute motion is not concerned. In cases where motion is concerned, at present I see very considerable difficulties in its application; but in the photographs obtained by Mr. Common of the nebula of Orion and in these photographs of star-clusters, and even in some cases of the photographs of the comet, provided you are indifferent to the effect of the comet's motion, it seems to me that we have an important advance in the application of photography to astronomical work.

Mr. Common presented his photograph of the great nebula of Orion. This was a Woodbury type enlargement of a photograph taken on Jan. 30, 1883, with the great three-foot reflector, and with an exposure of 37 minutes. Mr. Common considered that it showed a marked advance on previous photographs. Some of the finer details were lost in the enlargement, yet this showed several features not rendered in any drawing. Mr. Common added that photography would give the means of presenting the relative brightness of the different parts of a nebula better than the most careful hand drawing. And the question of changes in brightness and shape might also be decided by the same means. It would be necessary to compare photographs taken at some undetermined interval of time, and therefore the best thing now to be done was to take as many photographs as possible on a uniform system for comparison. The comparison of a photograph with a hand drawing would give no reliable evidence of changes in relative brightness, for obvious reasons. Mr. Common pointed out several differences between the photographs and the drawings of Lord Ross on the walls of the room. In particular, there was a long streak near the "Proboscis Major," which had not been previously observed, but the existence of which, after it had been indicated by the photograph, he had been able to verify with the eye. There were also several dark spots, which no drawing showed.

Mr. Common also thought that it would be possible to obtain photographs of stars too faint to be seen by the eye through the same telescope with which the photograph was taken.

Major Herschel. The nebula of Orion has been the subject of such a vast amount of labour by different observers, and so many drawings

have been made of it, that it is almost hopeless to discuss them. But there is one point on which I should like to ask a question. The photograph to which Mr. Common refers shows some dark spaces—gaps, holes, or lacunæ; I scarcely know what to call them. I should like to know whether he has attempted to see these through the telescope, so as to corroborate the photographs by actual vision, since they do not seem to be corroborated by any drawings that have been made.

Mr. Ranyard. I would like to make a remark as to the lacunæ referred to; they remind me of the cometary object which was seen cutting out the light of the corona photographed during the eclipse of 1871. There is a group of seven of these dark spots in a region to the south of the Sinus Gentilii—which seem to afford evidence of dark masses in space between us and the nebula.

Mr. Common. With regard to these dark spots, they can be accounted for in two ways. There may be portions of the nebula which have a colour different to that of the general mass, but which, though not perceptible to the eye, produces its effect on a photograph. This I think is hardly possible; but there is another consideration which has often struck me, viz. that we see this nebula from only one point of view. If we could look at it from another point of view, what we should see might be very different.

Thus we now see this “Proboscis Major” and these other great streaks lengthways; but if we could look at them from such a point of view as to see them “end on” as it were, their light would be all gathered together in close masses, and we might readily have just such dark openings left between them. It is impossible for us to see the real shape of the nebula, since we only get one view of it. With regard to seeing these dark spaces, it is, as I have said, not easy to recognize some of these appearances unless you know what they are like and are actually looking for them. Lord Rosse’s drawing is a most remarkable one, and, except in one particular, very exact. The relative intensity of the light is not properly shown. If you take any small portion of it, the drawing is correct as a picture of that particular portion, because in that case the bright parts are only comparable directly with the dark parts; but if you take two neighbouring portions and compare the relative intensities, they are all wrong. This is a difficulty inseparable from the nature of the object, because it cannot be all in the field of view at once; and the different intensities in different parts of it, and the differences of weather and definition under which drawings have been made, will account for a good many of the differences which they exhibit. But there are black spaces shown in all these drawings, and the only question is whether they are represented in their right places.

Mr. Rand Capron. I believe that Dr. Draper’s spectroscopic observations show that different parts of the nebula present different spectra. If so, that would imply that they had different photographic power.

Dr. Doberck. I should like to ask Mr. Common if it is possible to determine the relative brightness of stars by photographs, seeing that different rays of light from those seen by the eye produce the photographic impression. There cannot be a more important question than the possibility of there being dark stars in space, which has been asserted several times. These stars might emit light that might not be visible to the eye. But the first step towards the solution of that question would be to determine the magnitude of the stars from the photographic plates, and see if they bear the same proportions to each other as when seen by the eye, so that I would like to ask Mr. Common whether the relative magnitudes of the stars on the plates differ from their magnitudes as seen through the telescope.

Mr. Common. With regard to Dr. Doberck's question, as I have said, I have never had an opportunity of taking photographs of coloured stars, which is the only case where I can fancy that the photographic brightness would differ from the visual. As to the actual effect produced by a star on a photographic plate, there is no doubt it increases just as the magnitude does. The brighter the star is, the larger is the amount of silver which will be deposited on development. I have never seen any difference; but I have only been able this year, after years of work, to get such photographs as will at all compare satisfactorily one with the other, so that there has been no time for discussing these things; but, as far as I have gone, I should imagine that the diameter of a star on a photographic plate is in exact proportion to the intensity with which it strikes the eye. And I think that the colour of the star would not affect the image unless it affected the eye in a still greater proportion. That is one of those things that will have to be discussed at some future time; the only thing to do is to accumulate as many photographs as possible and discuss them carefully afterwards.

Mr. Ranyard. On the question of magnitude, I took the stars shown in Draper's photograph of the nebula, and put them down in the order of their apparent size, and found that they corresponded with the relative magnitudes as given by Bond, better than Bond's did with those given by Liaponor, so that the order of the relative brightness of the stars could be as well determined from the photographs as from the two catalogues. I do not think these dark marks on the nebula could be accounted for, as Mr. Common suggests, by a perspective view of long arms. They seem more like the coal-sack in the Milky Way, which can hardly be explained as a hole in the matter of the Milky Way. Even in the case of one such dark spot it seems more probable that it should be caused by a dark object between us and a bright background than that there should be a long tunnel in the matter of the Milky Way, and that the Earth should be so situated that we can just look through the hole; and when we find five or six of them, and all in the form of little circular dark objects, then the probability is much increased that there is something cutting out the light from behind;

in fact we have almost proof positive that there are dark objects in space.

On the announcement of a paper by *Mr. H. T. Vivian*, on the account of William Ball's observations of Saturn in the 'Philosophical Transactions,' the *President* remarked that the name in the papers of the Royal Society was spelt *Balle*, not *Ball*. Professor Adams had spelt it *Ball* without the *e*. It was important to bear this in mind, as otherwise some confusion might arise. Huyghens referred to him as "D. Balle," though his name was William Balle.

Mr. Dunkin. Perhaps "D. Balle" means "Dr. Balle."

The President. William Balle appears to have been one of the founders of the Royal Society, and was its first treasurer. He seems to have afterwards retired, probably to Devonshire, but to have kept up some communication with the Society. I may add that it seems to me that Prof. Adams is perfectly correct in saying that the design in question is not a drawing, but a paper cutting.

The following papers were also announced :—

J. Tebbutt. "Observations of the partial Solar Eclipse, November 10, 1882."

The Astronomer Royal. "Observations of Occultations of Stars by the Moon and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, in the year 1882."

E. F. Sawyer. "Observations of the Transit of Venus, 1882, Dec. 6, made at Cambridge, Mass., U. S. A."

Prof. D. P. Todd. "On the Observations of the Transit of Venus, 1882, Dec. 6, made at the Lick Observatory, Mt. Hamilton, California."

J. Williamson. "Observations of the Transit of Venus, 1882, Dec. 6, made at the Observatory, Kingston, Canada."

T. E. Espin. "The Variability of β Cygni."

J. E. Gore. "Notes on some of Schjellerup's identifications of Sufi's Stars."

W. F. Denning. "Note on Observations of Mercury."

Rev. S. J. Perry. "Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at Stonyhurst in 1882."

J. Tebbutt. "Note on Prof. Newcomb's remarks on the Windsor Observations of the Transit of Venus in 1874."

J. M. Schaeberle. "On the Magnifying-power of a Telescope."

T. H. Safford. "Notes upon certain doubtful Star-places."

The Astronomer Royal. "Observations of Comet *a* 1883, at the Royal Observatory, Greenwich, made with the Sheepshanks Equatorial."

Prof. C. V. Zenger. "On the Visibility of the dark side of Venus."

The following gentlemen were duly elected Fellows of the Society :—Rev. Edward Aurelius Adams, M.A., Major James Fellowes, R.E., Gerard Brown Finch, M.A.

The Meeting adjourned at 9^h 30^m.

*Account of Observations of the Lunar Crater Plato in
1879-82.*

[Continued from p. 91.]

South-east Area.

The Sector b.—In 1869-71 the sector was mostly seen of an almost uniform light tint; but in 1873-74 the appearance, as seen by Mr. Pratt, was very different, for several streaks were then seen traversing it, and the whole surface of the sector was apparently of various brightnesses. The recent observations add to the interest, for strange variations were seen. Thus its usual form was that shown in the chart; but frequently the eastern boundary only extended a little way east of spot 24, although, on Oct. 24, 1880, the Rev. F. B. Allison remarked of the sector, "broad, filled whole of S.E. corner." On March 6, 1879, Mr. Gray draws the sector with the south-west portion south of spot 17 wanting; and this portion has frequently been seen so faint as to be scarcely visible. Sometimes a dark triangular rift extended south-east from spot 17 to the wall at the letter *b*, and once or twice, in addition to this rift, another triangular dark marking cut off the extreme southern portion of the sector, as shown in the chart by the faint dotted lines. The portion of the sector south of *p* was also frequently fainter than the rest. These somewhat striking variations point not merely to change during a long period of years, but also to constant, perhaps almost daily, alterations.

North-east Area.

Streak η .—Remarkable for its great increase in breadth since 1869-71, especially near the border. On several occasions the breadth there was equal to the distance between spots 1 and 4.

Streak γ .—Increase in breadth striking, and, though one of the brightest streaks, it was not seen by Mr. Pratt in 1873-74.

Streak d^2 .—Known in 1869-71 as "the extension of the sector." Generally rather faint and narrow.

Streak a^2 .—The streak bears a strong resemblance to a^0 in length and breadth, and from its being seen to cross *over* the streaks in its path, thus indicating a more recent origin. Indeed there is some probability of its having been formed within the last ten years.

Streak b^2 .—The first indication of this marking occurs in a drawing by Mr. Gray, dated Jan. 12, 1881; and up to March 1882 it was only twice seen again, although it is not a very difficult streak.

North and North-west Areas.

Streaks α , σ , and κ .—Three streaks so intimately blended that they cannot well be considered apart. In 1869-71 α^0 varied very much in size and brightness, and similar variations are exhibited by the recent observations. Thus on May 11, 1881, it was as

bright as the sector and surpassed by no streak, whilst on July 12, 1881, it was rated below *b*, *i*, γ , *c*, and *p*.

Streak n.—Usually faint and narrow.

Webb's Elbow (i).—Generally one of the brightest streaks, but variable.

Streak v.—Seen faintly on two occasions only. Only seen once in 1869–71.

*Streak c*².—Seen once only; so perhaps a little doubtful.

Streak c.—Usually of the form shown in Mr. Elger's drawing on p. 285 of the 'British Association Report' for 1872.

Section III. General Remarks.

State of the floor.—For several days after sunrise the western portion of the floor generally had a peculiar white appearance, which rendered the streaks there invisible, and the same thing has been frequently seen on the east shortly before sunset, making the bright streak γ difficult to see. A similar appearance has been seen on the northern third of the floor of Ptolemæus, likewise causing the streaks there to become invisible. The convexity of the floor, though not always visible, was very frequently noticed, and once or twice a streaky appearance east and west. An interesting observation of sunset was obtained by Mr. Gray on July 29, 1880, when the interior was filled with shadow, with the exception of an irregular patch south of the centre, all trace of which disappeared at 14^h 30^m.

Spots seen as crater-cones.—Spots 1, 3, 4, and 17 were frequently seen as craters, sometimes under a pretty high illumination; 14 was only seen so on one occasion. Spots 7, 13, 18, 19, and 22 were observed as small elevated objects when on the edge of the shadow; but a crater origin could not be certainly determined. In this position 18 was very minute and only visible for two hours.

Brightness of the floor.—The variations in the brightness of the floor were considerable. Generally an observer watching Plato about full moon would see the floor very dark; but occasionally, under similar circumstances, the floor would appear quite bright, so as to be almost comparable with the same surface near sunrise, the streaks on these occasions being, if any thing, more difficult than usual. On the other hand, near sunrise and sunset the floor several times appeared unusually dark and perfectly smooth. Some portions of the floor often seemed much darker than the rest, particularly that part enclosed by the streaks γ , η , and b^2 .

Summary of results.—The results obtained from the recent numerous observations of this fine formation may be briefly stated as follows:—

(1) 38 spots were seen, as compared with 37 in 1869–71, seven of them being new.

(2) The mean visibilities of the spots of the two periods generally agreed very closely, excepting eight spots, Nos. 11, 12, 13, 14, 16, 22, 24, and 30.

(3) About 20 streaks were observed, three being new, and many streaks seen in 1869-71 were absent in 1879-82.

(4) Several streaks seen in 1869-71 were quite different in 1879-82, such as *p*.

(5) Many of the spots and streaks exhibited real and constant changes independent of the solar altitude and definition, occasionally such variations being very rapid.

(6) Considerable, and *real*, change has taken place in the state of the floor since the observations of 1869-71.

In conclusion, it may be mentioned that the observations are being continued with increased vigour. A somewhat important improvement consists in recording on every night of observation the relative brightness of all the spots and streaks visible according to a fixed scale, and this method promises to give valuable results.

West Brighton.

A. STANLEY WILLIAMS.

Instructions for the Observation of Variable Stars.*

A CONSIDERABLE number of observers have signified their intention of joining in the work, proposed in the pamphlet entitled "A Plan for Securing Observations of the Variable Stars" (reproduced in the 'Observatory,' Nos. 70 and 71). Since several of these observers are as yet without the requisite experience, some precautions are necessary to ensure such uniformity of method in the observations as will enable them readily to be reduced and compared together. The following recommendations, partly repeated from the pamphlet above named, are intended to assist in effecting this purpose. Other recommendations will be sent from time to time to the observers taking part in the work, if it seems desirable to do so.

It has not seemed best, however, to urge that all the observations should be made and recorded in exactly the same way, and to furnish printed forms for the records. The difference of instruments, and of the circumstances under which the observations are to be made, would probably make it difficult to secure precise uniformity. But there are some conditions which can be everywhere fulfilled, and the neglect of which will seriously impair the value of the work.

The time of each observation (hour and minute) should be recorded, and the date and place should be entered on each separate sheet of paper used in the records, as well as the signature of the observer.

For the completeness of an observation, it is necessary that the star observed should be compared both with a brighter and a fainter star. This may be done by either of the methods described in the "Plan for Securing Observations of the Variable Stars;"

* First Circular of Instructions for Observers of Variable Stars, acting in co-operation with the Harvard College Observatory.

that is, either by estimating the difference in brightness between the star observed, and each of the others separately, or by estimating its brightness upon a scale of 10, in which the number 0 represents the light of the brighter, and 10 that of the fainter comparison star. The method adopted should be stated, as the abbreviated form of the record may otherwise produce confusion. The estimates themselves may be made with photometric apparatus if the observer prefers; but he will then do well to add estimates made without the photometer, since very little time will be required to do so, as the star is already under observation. It may happen that the photometric observation will differ to some extent from the estimate.

When the stars observed are readily visible without a telescope, it is best to dispense with the instrument. Similarly a large telescope should not be used when a small one is sufficient. Neglect of this principle may seriously impair the accuracy of the observations.

Great care must be taken to identify with certainty the stars which are observed. It is obvious that an observer who has failed in doing so will practically lose all the time he afterwards employs in making estimates of relative brightness. Stars visible to the naked eye may be identified from any good atlas. The 'Atlas Cœlestis Novus' of Heis is especially to be recommended. American observers can obtain it through any importing bookseller, at a cost of about nine dollars. The 'Durchmusterung' (by Argelander) is the chief means we have for the identification of telescopic stars in the northern hemisphere. Observers unable to obtain it may often find it in a public library. With sufficient pains on the part of the observer, a telescopic star may be correctly identified by its place, with regard to some star visible to the naked eye.

The comparison stars must be clearly described by their positions with reference to well-known stars, or to the stars with which they are compared, either in words, or by a drawing showing the positions of the stars in the region, unless they have been identified from a catalogue, in which case the numbers of the catalogue suffice. In the record of the observations, the comparison stars may be conveniently designated by letters of the alphabet. In any set of comparison stars to be used with a given star requiring observations, the same letter should always be assigned to each on the different occasions when the region is examined.

The star requiring observation should also be described by its position with regard to the neighbouring stars, so that it may be known that the proper star has been observed.

It will not be necessary to repeat this work of identification on every occasion. When it has once been accomplished, and the region is known, a reference to the previous identification is sufficient in subsequent observations.

In any case, if a star is found equally bright with that to be

observed, the statement of the equality should be made, and furnishes one complete observation. But several independent observations on each date are highly desirable when they can be furnished.

If any one of the stars to be observed becomes too faint to be followed with the means at the disposal of the observer, notice of the fact should be sent at once to the Harvard College Observatory, in order that observations may be attempted with larger instruments.

In order to obtain satisfactory results, the observers are requested to pay close attention to the methods of observation described on pp. 10-13 ('Observatory,' No. 71, pp. 79-81) of the "Plan for Securing Observations of the Variable Stars," and to the form of record recommended on pp. 13, 14 ('Observatory,' No. 71, pp. 81, 82). The description of the stars employed in each region will probably require a separate sheet of paper. On the sheets containing the record of the comparisons a blank line should be left at the end of each series, and the full designation of the next variable star examined should have a line to itself, at the beginning of the comparisons relating to it. In these comparisons themselves it may be designated simply by the letter *V*.

Observers are also requested to make reports of their progress every month, so that additional instructions may be promptly sent if they are required. Even if no observations have been made since the last report, the statement of the fact will be of use in showing the actual state of the undertaking at given times.

Harvard College Observatory,
Cambridge, Mass., 1883, Jan. 27.

EDWARD C. PICKERING.

The Periodicity of Auroræ.

THE past year was remarkable amongst other events of interest for having presented to us three strongly marked instances of correspondence between the three phenomena of solar and magnetic disturbance and auroral display. That the *general* forms of the curves of solar spots, magnetic range, and auroral frequency were in close correspondence had been made evident before; but with the exception of the remarkable incident of 1859, September 1, there had been but little to favour the idea of a more intimate connection. The year 1882, however, afforded three examples of a particular correspondence, so close in each case, as to render the idea of chance coincidence quite untenable. In the case of the great spot of November, the magnetic disturbance commenced almost at the very hour when the spot appeared at the east limb, and continued to develop as the spot came further on the disk and increased in size; whilst auroral displays were witnessed on each night during the apparition of the spot, the most remarkable taking

place at the very time when it attained its greatest dimensions (real as well as apparent) and passed the central meridian.

It seems, therefore, a little startling, just at this time when the connection between the phenomena appears rather to be more than less intimate than we had supposed, to learn that a most painstaking observer of auroræ, situated too in an excellent place for watching them, has concluded that they are most frequent when there are no spots on the Sun, and rarest when the Sun is most active. Yet this would seem to be the deduction from fifteen years' observations made by Prof. S. Kleinschmidt at Godthaab, in South Greenland.

Prof. Sophus Tromholt, who has discussed these observations and collated them with whatever records he could find of similar work within or near the polar regions, and of whose results Dr. Terby has given an interesting summary in 'Ciel et Terre,' reminds us that the correspondence in period of the auroral frequency and solar and magnetic disturbance is based upon observations made wholly in the temperate zone, and he declares that "every thing that bears on the variable frequency of northern auroræ at Godthaab proves that things happen there in conditions entirely opposed to that which takes place in lower latitudes."

Prof. Tromholt has sought to eliminate the influence which the more and less cloudy state of the sky would exercise on the numbers of auroræ noticed, and during this investigation finds himself led to a conclusion quite opposed to the opinion of Weyprecht, who believed that the presence of a cloudy layer would be favourable to the formation of auroræ, for he finds "that the number of auroræ observed varies almost exactly in the inverse ratio of the quantity of cloud." Comparing the totals of days when auroræ were seen, and the same when an allowance had been made for the quantity of cloud, with Wolf's Sun-spot numbers it is evident that "not only does no parallelism exist between them, but, on the contrary, there is an almost diametrical opposition between the progress in frequency of auroræ and of sun-spots."

There are naturally but few records of auroral observations made near the polar circle, and these are mostly incomplete. But such as they are, Prof. Tromholt has examined them, and finds the observations made by Kleinschmidt at Godthaab, from 1865-1880, fully confirmed by those obtained at Jacobshavn, Sukkertoppen, Upernivik and Iviktut in Greenland, and at Stykkisholm in Iceland, at different periods from 1846 to 1880, and warrant him in concluding that in these latitudes the minimum of auroræ coincides with the maximum of sun-spots, and *vice versa*.

The opposition between the results obtained in the frigid and temperate zone is not confined to the eleven-year periodicity; it is seen also in the yearly variations. In lower latitudes a well-marked maximum has been remarked at the equinoxes; in the polar regions it takes places at the winter solstice. So, too, for the position of the displays. In Greenland they are almost always seen in the south, instead of in the north, as with us.

Weyprecht had already shown that the maxima at the equinoxes remarked in lower latitudes was due to an oscillation of the auroral zone towards the south at the time of the equinoxes, towards the north at the time of the solstice. In strict accordance with this view, zenith and northern auroræ at Godthaab were most frequent in winter, and least so at the equinoxes.

A similar oscillation was found to take place in the course of each night, the auroral zone travelling northwards, so that, of auroræ seen in the evening, the majority were situated in the south, but of those noticed in the morning, most were in the zenith or to the north.

The difference between polar and temperate regions as to the eleven-year period is explained in just the same way. As the maximum at Godthaab is approached, the proportion as well as the actual number of zenith or northern auroræ is increased; and it is because the auroral zone is then in its most southerly position that at the time of maximum solar activity auroræ are least frequent at Godthaab, and most frequent in lower latitudes. As the period of solar minimum approaches, the auroral zone travels northwards. So that "the three periods of the aurora (the eleven-year, yearly, and daily periods) all owe their origin and the difference witnessed in different places as to the epochs of their extreme terms to one and the same cause, the oscillation of the auroral zone from North to South."

Prof. Tromholt has investigated several other points of interest connected with the subject. He finds that the auroræ at the time of their maximum are more numerous, more extended, more brilliant, and show a greater variety of type than those of the time of minimum. He finds, too, that certain other phenomena related to auroræ have, like them, their maximum at the time of solar minimum, and the reverse. This is the case with cirrus; and he believes it to be the same for the magnetic perturbations, though at present positive evidence is wanting.

The opposition between high and lower latitudes is not confined to general auroral frequency; it is equally manifest in particular displays. "There is nothing at Godthaab to correspond to the principal auroræ of lower latitudes." Of the fine auroræ of the years 1869-1874, some two or three, at the best *slightly* manifested, were all that were seen at Godthaab, and on the night of 1874, Feb. 4, when the most superb display for several years was witnessed in England, Belgium, &c., there was absolutely nothing at Godthaab.

This curious drift or oscillation of the auroral zone strongly recalls the drift of the sun-spots towards the Equator in the period from maximum to minimum; and Prof. Tromholt enquires whether there is not a similar oscillation on Jupiter. On the whole, he remarks, that the time has not yet come for a satisfactory theory of this strange phenomenon to be stated. At present, observations are what are needed most; and with this view, a network of ob-

servations have been instituted to comprehend, Sweden, Norway, Denmark, Finland, England, and Ireland, and it has been already ascertained that even in the one country of Norway scarcely a day passes without an aurora being seen, a circumstance which impairs the value of all inferences based upon the fact that this or that event was concurrent with an aurora *somewhere*. Prof. Tromholt, during the last winter, has occupied the station of Kautokeino, in Finmark, for the purpose of making observations on the altitude of the aurora, which he believes to average 157 kilometres in height, in conjunction with similar observations made at Bossekop, nearly one degree due north of Kautokeino, and has satisfied himself that the auroræ seen there are identical with those he has observed in more southern latitudes. But the most notable advance of the last few months is due to Prof. S. Lemström, who has succeeded in producing an artificial aurora. He covered an area of some 900 square metres on the hill of Oratunturi, by what he has termed his "streaming apparatus," an uncovered copper wire with fine erected points at each half-metre. One end of this wire was connected by a covered copper wire with a zinc plate sunk in the ground at the foot of the hill. From this apparatus he obtained a yellow-white glow, which yielded a faint but perfect auroral spectrum.

Madeira Spectroscopic.*

IN May 1881 Prof. Piazzi Smyth went to Madeira to examine the more prominent groups of lines in the solar spectrum with a powerful spectroscope under favourable conditions. The spectroscope consisted of a Rutherford diffraction grating 1·6 inch square, having 17,296 lines to the inch, with collimator and telescope of 2½ inches aperture, used in connection with a heliostat and 6-inch achromatic lens of 90 inches focus. Magnifying-powers of 30, 45, or 64 were used on the spectrum of the second or third order. Special attention was given to the comparison of observations with a high and low Sun in order to obtain data for determining which of the lines were telluric in their origin. This may, in fact, be considered the most important feature in Prof. Piazzi Smyth's work; and the observations made at Madeira, where the atmosphere is charged with aqueous vapour, when compared with those made by the same observer in the dry climate of Lisbon, indicate with tolerable clearness those lines which are due to water-vapour and those which probably result from a dry gas in the Earth's upper atmosphere. The characteristic of true solar lines, as laid down by the author, is that they remain stationary in character as the Sun descends from the zenith to the horizon, and further, for solar

* Madeira Spectroscopic being a Revision of 21 Places in the Red Half of the Solar Visible Spectrum with a Rutherford Diffraction Grating at Madeira. By C. Piazzi Smyth. (W. & A. K. Johnston, Edinburgh, 1882.)

lines of the hottest kind, as those of hydrogen, magnesium, and sodium, a broadening and paleness of the lines accompanied with outside haze, produced by the explosions and various velocities of their incandescent vapours in various parts of the Sun's photosphere. On the other hand, the α band and the bands accompanying C and D on a low Sun, which are due to aqueous vapour, were resolved at Madeira into sharp black lines. The band α —which shows its telluric origin by its great increase in breadth and blackness as the Sun gets low, though we do not know what causes it—is similarly composed of lines of the thinnest, blackest, and best defined order. But though the Madeira observations show the same characteristic of sharply defined lines in the bands A and B, the former was so strikingly visible near the zenith, both at Lisbon and Madeira, that Prof. Smyth concludes that it must be a solar line of some external kind. This idea does not necessarily conflict with Capt. Abney's view enunciated at the British Association in 1882, that the B and A lines could not be claimed as telluric lines, much less as due to aqueous vapour, but must originate between the Sun and our atmosphere. As regards B; however, the Madeira observations show a very notable increase of intensity on a low Sun, which would indicate a partly telluric origin for this band, though it may also have its source in the outer solar regions, or even in the space between us and the Sun. However this may be, Prof. Smyth considers it is made absolutely certain by the Madeira drawings that neither A nor B have the characteristics of lines such as C, F, and the hydrogen line near G, which originate in the midst of the hottest influences, fiercest explosions, and most violent disturbances of the Sun's photosphere.

The results of the Madeira observations are given in seventeen plates photolithographed from Prof. Smyth's drawings. They represent the groups of lines near A, α , B, C, α , D, 1474 K, E, δ , F, and "near G." The micrometer measures have not been published, though they have been reduced to the form of tables exhibiting the observed position of each line as expressed on a scale of wave-numbers, *i. e.* number of waves in a British inch. It is to be hoped that funds may eventually be forthcoming for publishing these numerical results. On each plate are given the representations of the respective groups of lines by various authorities, commencing with Angström and ending with the Madeira results, the older drawings being magnified ten, twenty, or even forty times so as to bring them to the same scale. The comparison is instructive as showing the great improvement which has been made in spectroscopes since the times of Kirchhoff and Brewster and Gladstone, but it is somewhat unfair on careful observers who represented what they saw on a scale adapted to their instruments. However, making due allowance for the coarse look which Prof. Smyth's magnification gives to the delicate drawings of Angström and Kirchhoff, there is convenience in an arrangement which exhibits at a glance the progress in the delineation of detail in the

solar spectrum. And the comparison of such recent authorities as Capt. Abney, Prof. Langley, Prof. Young, Prof. Vogel, and M. Fievez with the Madeira results is specially valuable as showing the degree of accuracy which has been attained in mapping the Sun's spectrum.

The most striking feature of Prof. Smyth's drawings of the bands in the red part of the spectrum is the rhythmical arrangement of doublets of lines, suggesting, as the author remarks, that their positions may some day be mathematically computed. In the case of A the duplicity of the lines in the band was first detected by Prof. Langley, and was not perceived by Prof. Smyth till he used a narrow slit of such a width as to give the best definition in the brighter regions of B. Among other features of the series of plates we may notice the striking contrast between the band *a* of aqueous vapour, as seen in the dry air of Lisbon and the humid atmosphere of Madeira, the curious optical combinations formed by telluric lines near C with true solar lines, as seen with a low Sun, and the nebulous character of C, F, and the hydrogen line near G.

Besides the seventeen plates of portions of the spectrum there is a frontispiece showing clearly the distribution of the spectrum lines on the various scales of wave-numbers, wave-lengths, and the squares of wave-numbers, or prismatic spectrum-scale. The relative brightness of the different portions of the spectrum, as estimated by Prof. Smyth, is also indicated for each scale. At the end there is a coloured plate representing the colours of the spectrum, pure and in combination two or three together.

There can be no question as to the great value and importance of Prof. Piazzi Smyth's work as presented in the volume before us. Whilst other observers have expended their energies on the whole spectrum, he has concentrated his attention on certain portions of it, in which were groups of lines presenting the appearance of physical connection, and he has devoted the means at his disposal to mapping out, with all the exactitude attainable, their striking rhythmical arrangements. The regularity which characterizes his doublets of lines irresistibly suggests the existence of a law, for the discovery of which his observations afford valuable data.

W. H. M. CHRISTIE.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Relative Heights of Markings on Jupiter.

SIR,—

In your January number appeared a letter from Mr. Stanley Williams, in which an opinion is expressed that the darker

K 2

markings of Jupiter are at a higher level in the atmosphere of the planet than the white spots, or patches of light.

Being greatly interested in these markings, and holding what I believe is the generally received opinion, viz. that the light marks are uppermost, I have examined the drawings of more than twenty years, now numbering several hundreds, to see if they sustained the view advanced by Mr. Williams, and after the most careful investigation I fail to perceive the truth of this opinion. There are, on the contrary, many reasons for considering the light markings to be at a higher level than the darker. We will give a few of them :—

First, the general form of the light marks, these being round, oval, or compact patches, very unlike openings or rifts in a superficial cloudy envelope.

Second, that the oval forms which are so frequently seen on the equatorial side of the dark southern belt, indent equally both the dark belt and the general surface of the planet.

Third, that the continuity of a long dark streak is occasionally broken by a patch of light broader than the streak, the patch of light hiding therefore not only the streak but a portion of the general surface of the planet to the north and south of it.

The first reason, that of the general form of the light markings, may seem to be a weak one ; but, taken in connection with their relative position, is by no means inconclusive.

In March 1874 lines of small round patches of light, smaller than the satellites, were frequently seen, looking like strings of pearls ; these occurred generally on the dark southern belt, but were occasionally seen in northern and high southern latitudes. Now if the darker portions are uppermost these surfaces must have been pierced like the sides of a man-of-war, in order that the light underlaying portion might be seen through the openings. Again, in January 1873, large oval masses of light were so constant on the equatorial side of the southern belt, that the belt itself looked like a long dark bridge with many arches ; but let it be observed that these light forms not only indented the dark belt on one side, but equally indented the general tone of the planet on the other ; and if we consider the dark belt as being at a higher level, and the light marks as portions of a continuous light surface seen through its openings, we must admit that some other envelope is also pierced with similar openings, and that the two openings coincide, in order that the oval form may be complete—a supposition which is not recommended by its probability.

But the last argument, that of the imposition of a mass of light on a long dark streak, is the most conclusive ; this has occurred several times since the last opposition, the most marked instances being on Feb. 18, 8.55 G.M.T., and Feb. 24, 8.45. On the first date a broad and somewhat square patch of light interrupted the continuity of the darkest portion of the southern belt, and, being broader than the belt, extended in the direction of the equator

over the general tone of the planet. On Feb. 24, 8.45, a square patch of light was nearly on the centre of the disk; this lay on a long bluish streak. The patch of light was considerably broader than the streak, and, what is very remarkable, portions of blue streak appeared on the north and south sides of the square patch, as though the light patch had been formed from material drawn away from the general covering of the surface, thus leaving vacant spaces both above and below it.

These are some of the reasons for considering the light marks as being at a higher level than the darker ones; yet I would not argue that light markings are never seen under dark ones, but believe such occasions to be very exceptional.

I am, Sir, yours respectfully,

NATH. E. GREEN.

Telescopic Meteors.

SIR,—

On the night of March 2, 3, and 4, I observed five telescopic meteors of about 8-9th mags. They were noted in different parts of the sky while using a comet eyepiece on my 10-inch reflector.

I have been struck with the comparative slowness of motion of these and of many other telescopic meteors of similar type which I observed in 1881. I had formerly imagined that meteors, viewed telescopically, would be visible as mere transient flashes, except perhaps in the case of those moving slowly and belonging to streams overtaking the Earth in her orbit, which might no doubt be followed across the field of a low power. The swift meteors seen with the naked eye apparently traverse 10° or 15° of a great circle in about 0.2 or 0.3 sec., while many telescopic meteors, though evidently of the rapid streak-leaving class, and directed from radiant near the Earth's apex, travel with apparent slowness across the field of view. At the lowest estimate I believe they are usually 0.4 or 0.5 sec. in traversing 1° . The explanation appears to me that these faint shooting-stars are at vastly greater distances from the Earth's surface than those visible to the naked eye, and that their apparent slowness of movement and diminutive size are to be attributed to their great remoteness. They may be generally followed across the field of the telescope, though the nucleus is too small (and the phenomenon altogether too brief and unexpected) to show any details of structure. On one occasion, 1881, Oct. 3, 15^h 40^m, I saw an 8th mag. meteor in Leo, which left, for fully 65 seconds, a beautiful narrow streak, showing minute irregularities and reminding one forcibly of a spider's line on a frosty morning. On March 4 last, just before sunrise, I observed a telescopic meteor, evidently of the swift class, and tolerably near, for its velocity was too great for the nucleus to be followed. This meteor came out suddenly as a bright and extremely thin line across the field, lasting about 0.3 sec., and then disappearing without any

trace of a persistent streak. On another occasion I saw a telescopic meteor of about 12th mag. pursuing a curious whorl-like motion as it penetrated the atmosphere. My telescope, however, being simply an altazimuth, I have not been able to record the positions of these several objects with any degree of accuracy.

It would be interesting to attempt combined observations of telescopic meteors by different observers provided with suitable instruments. If some such plan could be successfully carried out our ideas as to the distances of these bodies might be vastly extended. It is also important that observers engaged in habitual comet-sweeping keep a record of the number of telescopic meteors and the length of observation. The three American observers, Messrs. Swift, Barnard, and Brooks, spend more than 100 hours per month* in comet-hunting, and ought to gather some important facts about telescopic meteors. A rough computation from my own somewhat meagre results shows that in a space of the sky about 50° square (such as may be well commanded by the naked eye) the hourly number of telescopic meteors is about 260, whereas the number of naked-eye meteors averages 12, so that the proportion of telescopic to naked-eye meteors is as 22 to 1. This, however, can only be regarded as a very rough approximation.

My results are based upon observations with a ten-inch reflector with two comet eyepieces having fields of $50'$ and $70'$, and employed to about the same extent, so that the average field would be about $61'$. The subject is a very interesting one for further research, and can be readily pursued by comet-seekers without interfering with their main purpose. It is therefore to be hoped that this detail of meteoric astronomy will now receive some attention as one likely to enlarge our views both as to the numbers and distances of the myriads of shooting-stars daily encountering the Earth.

Yours faithfully,

W. F. DENNING.

Bristol, 1883, March 8.

Serles's Ephemeris.

SIR,—

M. Souchon, in his recently published excellent *Treatise on Practical Astronomy* with an Introduction giving a history of the progress of the formation of ephemerides, refers to John Searle's as the first work of the kind published in England, containing an astronomical ephemeris for the years 1609 to 1617. I have had the curiosity to examine this work, of which there is a copy (without date or titlepage) in the British Museum. As may be supposed, it is exceedingly quaint and plentifully interlarded with astrological rules and directions. The author's name, as printed in the book, is not spelt as M. Souchon spells it, but John Serles, who is desig-

* This statement is based upon information in the 'Science Observer,' Nos. 39-40, p. 30, where we find that during the four months, May to August 1882, the three observers named were employed in systematic sweeping to an aggregate of 412 hours, or 103 hours per month.

nated as "Master in Chirurgie." The worthy John gives the etymology of Ephemerides, which he treats as a word in the singular, "importing as much as Almanack in the Assirian tongue, Dioicum or diurnales in the Latin, and as Register, Diarie, or Journall in the English tongue." Then after stating that all previous ephemerides were "false, imperfect, and erronious," he informs his readers that "The reformation whereof, not any hitherto have attempted, but only the *Tresnoble Tycho Brahe*, the *Phoenix* and *Mirrou* of all moderne *Astronomers*, who notwithstanding before he could accomplish it, was prevented by death, leaving onely most excellent and exact observations, and an adumbration of his *Hypotheses*, from which chiefly in the motions of the sunne and moone, as also from the best observations of others our modern *Astronomers* in the other planets, I have deduced these *Ephemerides*."

Amongst other interesting information, this curious work contains a list of approximate latitudes and longitudes of "the most notable citties in divers regions" (including such as "Saba in Arabie the desert," and "Saba in Arabie the happie") and of the thirteen most remarkable "Citties and townes of Note within this Empire," which are reckoned to be Cambridge, Canterburie, Colchester, Barwicke, Bristol, Leicester, Lincolne, London, Hereford, Oxford, Norwich, Northampton, and Yorke. It is noteworthy that in the ephemerides are given the dates according to both old and new style or (as they are called) "the olde Kalender used in England" and "Foreign acctpt." Can any of your readers give any account of the author?

W. T. LYNN.

Blackheath, 1883, Feb. 22.

Brightness of Pollux.

SIR,—

On referring to old star catalogues, it is somewhat remarkable to find Pollux rated inferior in brightness to its neighbour Castor. The most cursory eye estimation at the present time will show the first-named star at the very least $\frac{1}{2}$ magnitude the brighter. Are we to suppose Pollux has brightened in this time? De la Caille, Zach, and Mayer give as follows:—

	α Geminorum.	β .
La Caille.....	1	2 (magn.).
Zach	1, 2	2 „
Mayer	1, 2	2 „

Again, in Bode's map (1782), Castor and Pollux are represented as equal in brightness. At the present day α is not very much below Regulus in brilliance, nor β much inferior to Aldebaran, so that to call α a bright 2nd magnitude, and β an ordinary 1st will not be far wrong. The maps of the S. D. U. K., while putting Pollux the brighter, underrate both, giving Castor = 3, and Pollux = 2 (magn.).

Faithfully yours,

S. J. JOHNSON.

The Star of 1572.

SIR,—

My attention having been recently again called to this famous star, of which Tycho Brahe gave so full an account, by a paragraph in one of the daily papers, stating that "astronomers" expected its return this year or next, I was led to examine whether there is any real evidence for its having been seen before 1572. We all know that Sir John Herschel, in the 'Outlines' expressed a belief in the probability, or at least possibility, of this star being identical with new stars said to have been seen near Cassiopeia in the years 945 and 1264. Pigott (not, I think, Goodricke as Sir John says) suggested, about a century ago, the identity of the three "stars." Now Tycho, in his well-known treatise on the star of 1572, discusses the repeated appearance of a previous one in 1264. He states that it was related by Cyprian Leowitz (Leovitiu) on the authority of no published work, but of some manuscript of which Leowitz did not mention the author's name. And he argues that it is impossible to accept it on such authority, for it is almost incredible that no historian whose writings had been published should have mentioned so remarkable and unusual a heavenly visitor. It appears to me that Tycho's argument is unanswerable; but I have had the curiosity to consult Leowitz's work '*De Conjunctionibus magnis insignioribus superiorum planetarum, solis defectionibus et cometis &c.*,' for the purpose of seeing what he does say on the subject. There is absolutely no mention of any such star in it, nor does any other published work of Leowitz bear on the subject. I conclude therefore that Tycho referred to some paper or letter by that author which he had seen in manuscript. Slight indeed, then, is the foundation for the appearance of any new star in 1264, independently of any discussion which we might have of its place if we were sure of its existence.

May I take the opportunity of suggesting that it might be interesting from time to time to observe any star near the place which would now be the most probable position of Tycho's star? Argelander, I need not remind your readers, determined this carefully, so far as it was possible from Tycho's data, reducing the place to 1865, which he found to be approximately

R. A. $0^h 17^m 20^s$. N. P. D. $26^\circ 36'$.

D'Arrest had observed a star of the $10\frac{1}{2}$ magnitude very near this place, preceding it by about half a minute of time. But I do not know whether any one has tried to observe it since. Perhaps the deserved popularity of Guillemin's 'Heavens' may make it worth while to remark that in the last English edition (edited by Mr. Lockyer and revised by Mr. Proctor, published 1876) the right ascension is erroneously given as $4^h 19^m 57^s.7$, these figures really referring to the place in arc as Argelander has given them, $4^\circ 19' 57''.7$.

Yours faithfully,

W. T. LYNN.

Blackheath, 1883, March 22.

NOTES.

HARVARD COLLEGE OBSERVATORY.—The Annual Report of this Observatory contains, as usual, several features of interest. Great progress has been made in the reduction of past observations. The volume containing the micrometric measures made with the large telescope to the beginning of 1882 has been distributed. The reductions of the zone observations made from 1871 to 1879 are approaching completion; the meteorological observations from 1840 to 1880 have been brought together, and are nearly ready for publication; and a catalogue giving the results of the photometric observations of four thousand stars is now in the hands of the printer.

The large equatoreal has been used, as in former years, in the photometric observations of the eclipses of Jupiter's Satellites, the search for objects with singular spectra (a search resulting in the discovery during the past year of seven new planetary nebulae), photometric observations of the Satellites of Mars (which were seen as long after opposition as February 24), of various points on the Moon, of certain double stars, and of Sawyer's Variable. The determination of the brightness of the standard points on the Moon has been completed, and it appears that the scale of brightness in common use by observers of the Moon might be very closely expressed in terms of stellar magnitudes, each degree of the scale answering to the ratio of light equivalent to six tenths of a magnitude. An interesting case of the suspected variability of a double star is afforded by θ Serpentis, the components of which usually differ by 0.5 or 0.4 of a magnitude, but on one evening a difference of 1.4 was noted. If this observation was correct, the star would probably belong to the Algol type.

The telegraphic system for the speedy transmission of various data respecting comets has been maintained and extended. In the case of the great comet of last September, cloudy weather prevented sufficient data for an orbit being secured in the first few days of its appearance; but on receipt by telegraph from Europe of one more position, the orbit was computed, and telegraphed back within seven hours.

The measurement of the light of the stars visible to the unaided eye with the meridian photometer, a work involving over 90,000 measures, was completed in the summer of 1882. Amongst other results, the discussion of about 15,000 observations shows that the atmospheric absorption may be assumed, for any altitude exceeding 15° , to equal in stellar magnitudes one quarter of the secant of the zenith distance. The new large meridian photometer has been completed, and work with it commenced. The bibliography of the variable stars undertaken by Mr. Chandler last year has been nearly completed, so far as the preparation of the list of references is concerned, and a plan has been prepared for securing co-operation in the observation of these objects.

As mentioned in the 'Observatory,' No. 70, p. 64, the increased efficiency of the Harvard College Observatory during the last five years has been rendered possible by the liberality of several gentlemen who together subscribed 5000 dollars a year for five years. This subscription expires in the present year, and an attempt is now being made to raise 100,000 dollars to form a permanent endowment. Of this amount 30,000 dollars have already been promised, on condition that 75,000 dollars shall be secured before next September. The well-known liberality of the people of Boston and their intelligent interest in all scientific progress leave little doubt that Prof. Pickering's appeal will meet with a gratifying response.

SIR WILLIAM HERSCHEL'S OBSERVATIONS OF STELLAR MAGNITUDE.—The following reference to Sir William Herschel's observations in the above report will, we think, be interesting. Prof. Pickering says:—"A reduction of the observations of Sir William Herschel has been effected, and has led to interesting results. His observations of the light of the stars are not only far superior to any similar work preceding it, but are more precise than most of the subsequent determinations. Their neglect hitherto is partly due to the want of a suitable system of magnitudes by which they might be reduced. This want is supplied by the photometric measures now under consideration. There seems also to have been an impression that the intervals employed by Herschel were so large as to render the observations uncertain. Our reduction shows that the intervals he designated as a period, comma, and dash, do not exceed one, two, and four-tenths of a magnitude; and that the average deviation of a single comparison of two stars, expressed in magnitudes, is only 0.25. As this includes the error of our measurements of each star, the difference of each as seen by the eye from its true brightness, and the variation each has undergone during the past century, it is obvious that the errors of Herschel's observations must be very small. We have thus an accurate measure of the brightness of a large part of the lucid stars a hundred years ago. This will be of the utmost value in determining any changes of long period that may take place in their light."

THE SELECTIVE ABSORPTION OF SOLAR ENERGY *.—Prof. Langley has recently given, in the 'American Journal of Science,' a somewhat fuller account of his bolometric observations at the Allegheny Observatory and on Mt. Whitney than has previously appeared. In this paper, Prof. Langley describes the instrument, the spectro bolometer, with which his researches were effected, and after explaining his methods of observations and his mode of reducing

* Amer. Journ. of Science, Vol. xxv. No. 147.

them concludes with a summary of the principal results at which he has arrived.

Some of these have been already published in the 'Comptes Rendus' and elsewhere, and were referred to in the 'Observatory,' No. 67, p. 340. The most striking result was the revelation of an hitherto wholly unexplored region of the spectrum in the infra-red, lying not merely out of sight, but beyond even the limits which Capt. Abney's photographs had disclosed to us. We are accustomed to speak of the two unseen regions of the spectrum, the ultra-violet and the infra-red, in much the same terms as if they were of about equal importance. But the infra-red rays represent nearly 100 times the amount of energy as the ultra-violet, and their extent in the normal spectrum is nearly eight times that of the whole of the spectrum ordinarily visible to the eye. Indeed, roughly speaking, three-fifths of the entire solar energy which passes through our atmosphere has its seat in this part of the spectrum.

Outside our atmosphere, however, the proportion of the total heat belonging to the infra-red rays is much smaller, for, contrary to the opinion formerly held, the absorption of the atmosphere seems to increase systematically from the infra-red towards the ultra-violet. The maximum heat outside the atmosphere is in the very centre of the luminous spectrum in the green between wave-lengths 500 mmm. and 550 mmm., whilst at the sea-level it is at 550 mmm. or, as the Sun descends to the horizon, at about 650 mmm. Since the blue rays suffer a greater loss in their passage through the atmosphere than the red and yellow, it follows that what we term white light "is *not* the sum of all radiations, but only of a part, even of the visible ones," and that were our atmosphere removed the Sun would at first appear to be of a decidedly bluish tint. "Since, too, the heat in the shorter wave-lengths (corresponding in a general sense to high solar temperature) was thus relatively greater before absorption, we are obliged to increase our usual estimates, not only of the amount of heat the Sun sends us, but (and very greatly) of the effective temperature of the solar surface."

VARIABLE STARS*.—Prof. Schmidt has recently published the results of his observations of variable stars made during the year 1882. These include observations of seventy-six stars. Amongst the stars observed are the following:—1. δ Libræ: minima, May 17^d 10^h 17^m.8 Athens mean time, and June 14^d 10^h 0^m.8. 3. Algol: minima, Nov. 29^d 11^h 30^m.4 and Dec. 2^d 8^h 7^m.1. 10. Mira Ceti was at minimum on Feb. 4, magnitude 9.5. 11. χ Cygni was at maximum Sept. 1.5, magnitude 6.1. 13. α Herculis was at maximum Feb. 20, June 24, and Sept. 19; and at minimum April 7, Aug. 6, and Nov. 3. 30. δ Cancri on April 14 at 10^h.1 and 11^h.3

* Astr. Nach. No. 2491-92.

was 11.7 magnitude, fainter than ever Prof. Schmidt had seen it before. 35. T Cephei was at maximum on Jan. 11, magnitude 6.7, the increase of light being much quicker than the decrease. 39. U Coronæ was at minimum Nov. 6, 6^h.6. 40. V Coronæ; maximum Sept. 15.6, magnitude 8. 45. Nova Cygni of 1876 was looked for repeatedly without success. 53. R Leonis: maximum May 20, magnitude 6.5; minimum Nov. 6, magnitude 9.

Ceraski's Variable, D.M. 81° No. 25, showed minima on May 2^d 11^h 6^m.2, May 7^d 10^h 27^m.0, Nov. 25^d 8^h 57^m.2, and Nov. 30^d 8^h 36^m.5. Mr. Knott, we learn from 'Nature,' secured a good observation of this star on March 2 in the present year. The observed time of minimum was 12^h 36^m, and the magnitude 9.45. The low magnitude attained confirms, Mr. Knott thinks, his previously expressed opinion that it shows a somewhat lower magnitude every alternate minimum than at those intervening.

μ DORADÛS*.—The star μ Doradûs, long supposed to be variable, has been proved by Dr. Gould to have no real existence at all, and to have owed its origin to a mistake of Lacaille's, by which six stars had their right ascensions recorded one hour too small. Brisbane found a star near the place thus erroneously assigned to the star Lacaille had called μ Doradûs; and as this was found by Moesta to be of magnitude 8½ or 9, instead of the 6th, as in Lacaille's catalogue, he watched it carefully for five years in order to detect some fluctuation in brightness, but without being able to note any change. The star Lacaille had really observed was 47 Pictoris. A similar error affects the places of five other stars observed by Lacaille on the same evening, Nos. 1542, 1554, 1633, 1689, and 1706 of Lacaille's Catalogue published by the British Association.

A NEW COLLIMATOR†.—MM. Thollon and Gouy have devised a new form of collimator in order to obviate the necessity for turning the spectroscope whilst observing solar prominences. A prism, with its hypotenuse parallel to the axis of the collimator and also to the slit, is placed just behind the latter. The light undergoes total internal reflection from the hypotenuse and an equal amount of refraction, but in opposite senses, on entering and on leaving the prism, the sole resulting effect being to change right for left and *vice versa*, in other words, it is a Zollner reversion prism. The slit and prism are so connected that when the former is revolved through any angle, the latter is turned through half that angle; the image of the slit therefore remains in one position throughout.

* Astr. Nach. No. 2498.

† Comptes Rendus, Vol. xvi. No. 10.

COMET *a*, 1883.—This object, which is now quickly diminishing in brightness, was at the beginning of March tolerably bright, nearly round, and with a very condensed nucleus. According to some observers, it had a granular appearance like a resolvable nebula. Its tail was faint and very straight and narrow, some 15' or 18' in length, its position-angle on March 2 being very nearly 11°.

The spectrum of the comet showed the usual three bands coincident with the three principal bands seen in the spectrum of a Bunsen flame.

THE NEW HONG KONG OBSERVATORY.—Dr. Doberck, from Col. Cooper's Observatory, Markree, Ireland, has been appointed to the Directorship of the new magnetic and meteorological observatory about to be established at Hongkong.

Elements and Ephemeris of Comet a, 1883.*

By DR. H. OPPENHEIM.

THE elements are computed from observations on 1883, February 24, at Cambridge, Mass., on March 2 at Hamburg and Kiel, and March 8 at Kiel. Parallax and Aberration have been taken into account.

Perihelion passage = 1883, February 19·1234, Berlin M.T.

Long. (Per.—Node)....	111° 10' 42"	} Mean Equinox 1883·0.
Long. Per.....	278 14 29	
Inclination.....	78 2 8	
Log. <i>q</i>	9·88086	

The representation of the middle place is :—

$$\Delta\lambda \cos \beta = +11''; \Delta\beta = +20''.$$

Ephemeris for Berlin Midnight.

<i>t</i> , 1883.	<i>a.</i>			<i>δ.</i>	Log. <i>r.</i>	Log. <i>Δ.</i>	<i>I.</i>
	<i>h</i>	<i>m</i>	<i>s</i>	' ''			
Mar. 31....	3	34	13	+21 15·1	0·0338	0·1798	0·30
Apr. 1....	3	38	40	20 45·7			
2....	3	42	59	20 16·6			
3....	3	47	10	19 47·9			
4....	3	51	14	19 19·6	0·0541	0·2020	0·25
5....	3	55	11	18 51·8			
6....	3	59	2	18 24·3			
7....	4	2	46	17 57·3			
8....	4	6	25	+17 30·6	0·0740	0·2237	0·20

The brightness at time of discovery is taken as unity.

* Dun-Echt Circular No. 73.

Approximate Times of Transit of the great Red Spot and Equatorial White Spot across the Central Meridian of Jupiter in April 1883.

Date.	Red Spot on C.M. h	Date.	White Spot on C.M. h
April 1	7.3	April 2	8.8
3	8.9	4	10.0
5	10.6	6	11.2
8	8.1	7	6.9
10	9.8	9	8.0
13	7.3	11	9.2
15	8.9	13	10.4
17	10.6	16	7.3
20	8.1	18	8.5
22	9.8	20	9.7
25	7.3	22	10.8
27	8.9	25	7.7
29	10.6	27	8.9
May 2	8.1	29	10.1

On March 15 the red spot was observed by the writer as central at 8^h 13^m, and the white spot came to the C.M. at 8^h 16^m, or 3^m later. The conjunction of the two spots must therefore have occurred at 13^h 38^m March 15. It may be interesting to note that since Nov. 19, 1880, 9^h 23^m, the white spot has completed 19 revolutions of Jupiter relatively to the red spot, the number of rotations being 2045 for the red spot, and 2064 for the white spot. The latter moves some 260 miles per hour swifter than the former, in a direction from W. to E. around the planet.

The following recent observations of the red spot have been obtained here:—

1883.	On C.M.		Marth's 1st meridian.		Spot follows 1st meridian.	
	h	m	h	m	h	m
Feb. 24	7	30	6	3.9	1	26.1
Mar. 2	12	26	10	58.8	1	27.2
10	9	5	7	36.6	1	28.4
15	8	13	6	44.8	1	28.2

In July 1881 the spot followed the first meridian 8^m; but during the 1 year 8 months which has elapsed, the interval has increased to 1^h 28^m, so that the spot has been losing 4^m per month upon the period of 9^h 55^m 34^s.42 adopted by Mr. Marth. This retarded motion amounts to 8 seconds per day, and to 3.3 sec. in one rotation, so that the average period of the red spot since July 1881 has been 9^h 55^m 37^s.7. The extreme faintness of this remarkable object, and the fact that Jupiter is now rapidly nearing the Sun, render it important that the spot should be critically watched during April, as this may be the last month of its visible existence.

Bristol, 1883, March 16.

W. F. DENNING.

Astronomical Memoranda, 1883, April.

Sun. April 1, sets $6^h 30^m$, rises $17^h 36^m$; April 11, sets $6^h 47^m$, rises $17^h 13^m$; April 21, sets $7^h 3^m$, rises $16^h 52^m$; April 30, sets $7^h 18^m$, rises $16^h 34^m$.

Equation of Time:—*Sun after Clock*, April 1, $3^m 59^s$; April 11, $1^m 6^s$; *before Clock*, April 21, $1^m 18^s$; April 30, $2^m 52^s$.

Sidereal Time at Mean Noon:—April 1, $0^h 38^m 0$; April 11, $1^h 17^m 4$; April 21, $1^h 56^m 9$; April 30, $2^h 32^m 3$.

<i>Moon.</i>	<i>rises.</i>			<i>sets.</i>			<i>sets.</i>	
	h	m		h	m		h	m
Apr. 1..	14	40	Apr. 11..	11	53	Apr. 21..	16	40
2..	15	13	12..	12	44	22..	<i>rises.</i>	
3..	15	43	13..	13	25	23..	8	29
4..	16	9	14..	13	58	24..	9	30
5..	16	37	15..	14	26	25..	10	25
6..	17	5	16..	14	50	26..	11	16
7..	<i>sets.</i>		17..	15	13	27..	12	0
8..	8	22	18..	15	34	28..	12	39
9..	9	40	19..	15	54	29..	13	13
10..	10	51	20..	16	16	30..	13	41

New Moon, April 7, $1^h 36^m$; First Quarter, April 13, $20^h 50^m$; Full Moon, April 21, $23^h 27^m$; Last Quarter, April 29, $19^h 3^m$.

The Moon is in conjunction with Venus, April 4, 6^h ; with Mars, April 5, 7^h ; with Mercury, April 6, 12^h ; with Saturn, April 9, 13^h ; and with Jupiter, April 11, 18^h .

There will be a partial eclipse of the Moon, invisible at Greenwich, on April 22. Middle of the Eclipse, April 21, $23^h 39^m$. Magnitude of the Eclipse (Moon's diameter = 1) 0.085 .

Mercury is an evening star towards the end of the month. He is in superior conjunction with the Sun, April 15, 23^h , and in conjunction with Neptune, April 26, 20^h .

Venus is a morning star throughout the month. Diameter:—April 1, $16''.3$; April 30, $13''.6$. Illuminated portion of disk, 0.735 on April 15.

Apr. 1, R.A. $22^h 5^m.3$, Dec. $12^\circ 1'$ S., tr. $21^h 28^m$, rises $16^h 25^m$
30, 0 13 .8, 0 15 S., 21 42 15 36

Mars is in perihelion, April 13, 3^h .

Jupiter is in Taurus throughout the month. Diameter:—April 1, $34''.5$; April 30, $32''.0$.

Apr. 1, R.A. $5^h 36^m.1$, Dec. $23^\circ 15'$ N., tr. $4^h 57^m$, sets $13^h 13^m$
30, 5 56 .3, 23 25 N., 3 24 11 41

Saturn is an evening star in Taurus.

Apr. 1, R.A. $3^h 26^m.4$, Dec. $16^\circ 50'$ N., tr. $2^h 48^m$, sets $10^h 22^m$
30, 3 40 .2, 17 42 N., 1 8 8 47

		Outer Ring.		Inner Ring.		Ball.
		Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Apr. 9		37''·85	15''·28	25''·17	10''·16	15''·1
29		37'·30	15'·37	24'·80	10'·22	15'·0

The south side of the rings is visible, the elevation of the Earth above their plane being $23^{\circ} 49'$ S. on April 9, and $24^{\circ} 20'$ S. on Apr. 29, and of the Sun $24^{\circ} 21'$ S. and $24^{\circ} 30'$ S.

Uranus is about $20^m p$ and $2^{\circ} n$ of β Virginis.

Apr. 1, R.A. $11^h 26^m \cdot 2$, Dec. $4^{\circ} 31' N.$, tr. $10^h 46^m$
 30, 11 22 ·7, 4 53 N., 8 49

Phenomena.

G. M. T.			G. M. T.		
h	m		h	m	
Apr. 4	9 34	J. iv. Ec. D.	Apr. 14	9 27	J. i. Tr. E.
	11 0	J. iv. Ec. R.	15	7 53	J. i. Ec. R.
5	10 43	J. i. Tr. I.	21	9 10	J. i. Tr. I.
	11 32	J. ii. Tr. I.	22	9 48	J. i. Ec. R.
6	8 0	J. i. Oc. D.	23	9 12	J. ii. Tr. E.
	11 28	J. i. Ec. R.	12	55 28	Libræ
7	8 20	J. iii. Oc. D.			Oc. D. 140° .
	11 11	J. ii. Ec. R.	13	32 28	Libræ
	11 11	J. iii. Oc. R.			Oc. R. 192° .
12	8 32	J. iv. Tr. E.	25	9 56	J. iii. Tr. E.
13	8 20	λ Geminorum	26	14 13	B. A. C. 6081
		Oc. D. 25° .			Oc. D. 118° .
	9 6	λ Geminorum	15	27	B. A. C. 6081
		Oc. R. 304° .			Oc. R. 246° .
	9 59	J. i. Oc. D.	29	8 28	J. i. Oc. D.
14	8 44	J. ii. Oc. D.	30	9 12	J. ii. Tr. I.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

Publications received:—J. R. Young, *Navigation and Nautical Astronomy* (Crosby Lockwood & Co., London)—*List of Longitudes, Latitudes, and Altitudes* (Government Press, Washington)—Col. J. F. Tennant, *Valuations of Coins which are now or have recently been current*—B. J. Hopkins, *Observations of the Great Comet* (b) 1882 (Monthly Notices R. A. S.). Also the following to 1883 March—*L'Astronomie* (Gauthier-Villars, Paris)—*Sirius* (Scholtze, Leipzig)—*Ciel et Terre* (F. Hayez, Brussels)—*The Sidereal Messenger* (Northfield, Minn.)—*The Analyst* (Des Moines, Iowa)—*L'Union Scientifique* (Amiens)—*Memoire della Società degli Spettroscopisti Italiani* (Eredi Botta, Roma)—*Bollettino Mensuale dell' Associazione Meteorologica Italiana* (Torino).

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 73.

MAY 1.

1883.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1883, April 13.

E. J. STONE, M.A., F.R.S., *President*, in the Chair.

Secretaries: J. W. L. GLAISHER, M.A., F.R.S., and
E. B. KNOBEL.

THE Minutes of the last Meeting were read and confirmed.

Mr. Knobel announced that 69 presents had been received since the last meeting of the Society.

Captain Noble read a note on the Eclipse of Jupiter's Fourth Satellite on April 4, 1883. He said his note was scarcely sufficiently important to be entitled to be called a paper, its purpose being simply to point out that the satellite did not begin to fade until some minutes after the predicted time as given in the 'Nautical Almanac.' The satellite also kept flashing out at intervals in a way which he thought could not be wholly explained by atmospheric undulation. The final disappearance did not take place until $9^h 58^m$ G.M.T., or $24^m 6^s$ after the time predicted in the 'Nautical Almanac,' viz. $9^h 33^m 54^s$. He suggested, therefore, that a note should be inserted in the "Explanation" at the end of the 'Nautical Almanac,' pp. 501 and 502, to the effect that these predictions might be 24 minutes in error. The observation was made with a power of 154 on his Ross equatoreal of 4.2 aperture.

Mr. Hind. I may say, with regard to the remark of Capt. Noble, that I have heard of the observation, but have hitherto had no opportunity of ascertaining if there is any slip in the 'Nautical Almanac' prediction. The tables of this fourth satellite are notoriously very imperfect. With regard to Capt. Noble's suggestion, that we should give notice of what the probable errors of the predictions are likely to be, I think that would be entirely out

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of the question. We must hope that in time we shall have these tables more correct. The numerous observations which are now made of the satellites, and the Damoiseau prize that is offered by the French Academy of Sciences, may, I hope, lead eventually to a real improvement in these tables.

The President. You are referring also to the third satellite, I suppose. The tables of the third are not so bad as those of the fourth, but they are also a good deal out, I believe.

Mr. Hind. Yes.

Mr. Howlett. I was watching this eclipse, and it seemed to me that it was fully six minutes after the predicted time before the slightest diminution in the light became visible. It then soon became a very delicate object and visible with great difficulty. Ought it to have totally disappeared?

Captain Noble. Clearly it should have gone out.

Mr. Howlett. What then is the reason of its remaining just visible for many minutes? I can bear witness it was six minutes beyond the predicted time before the diminution of light commenced, and it was visible for many minutes after that, though as a most delicate object.

Captain Noble. I should like once again to draw attention to the way in which the satellite kept flashing up. When I thought I had lost sight of it altogether, it would flash out again with comparative brightness. Of course scintillation or something of that kind was the principal cause, but I could not persuade myself that it was wholly referable to that.

Mr. Howlett. The definition was very bad whilst I was observing the phenomenon.

The President. I am afraid this complaint about the uncertainty of the predictions with regard to these satellites is nothing new. It does not rest with Mr. Hind, the superintendent of the 'Nautical Almanac,' but with the people who will not make new tables. Though the predictions are of very little use for determinations of longitude, still, I suppose, it is useful to direct the attention of observers to the occultations and eclipses.

Mr. Knobel read a communication from Mr. Gill, from the Cape Observatory, on the nucleus of the Great Comet of last September. This paper related to a point mentioned at the December meeting of the Society, as to whether any duplicity had been observed in the nucleus of the comet before perihelion. Mr. Gill said that no duplicity was noticed until September 30. On September 7, 8, and 9, the comet presented a sharp, well defined disk. An interval of cloudy weather then intervened, but on September 17 the nucleus was still single and under 4" in diameter. Up to September 29 the comet presented the appearance of a star of the first magnitude; but on September 30 Mr. Finlay described it as showing two balls of light in the head. Later on, several bright points were noticed. On October 10, four bright defined points were seen connected by a line of light like beads on a string, only that the

connecting light was fuzzy, and broader than the nuclei. On October 17, 5 points were noticed. The nucleus attained its maximum apparent size in December; and on March 6 the comet was still a conspicuous object in a good opera-glass.

Mr. Knobel also read a note on *Mr. Finlay's* preperihelion observations of the Great Comet.

Mr. Lecky gave an explanation of two small transit instruments designed by *Mr. Latimer Clark*. They were, he said, simple in form and very useful in a certain way, but were not intended to be accurate to the 100th of a second, but would take the time to within half a second. The instruments were exceedingly well made; the axis was solid and everything substantial. The setting of the instrument in declination was very simple. The instrument was lifted off the supports, and a handle moved a vernier round the circle to the required reading. The instrument was then replaced on the supports, and the handle came against a stop when it pointed to the declination required.

The President. Are you supposed to lift out the instrument every time you use it?

Mr. Lecky. Yes; but the whole thing is so substantial that there is little danger of change. The illumination is a simple lantern throwing the light on a little white reflector which illuminates the wires—three vertical wires and one horizontal. *Mr. Latimer Clark* has prepared and published an elaborate treatise to be used with the instrument; and the mean times of the transits of a number of stars are worked out very accurately.

Mr. De La Rue. What is the cost of the instrument?

Mr. Lecky. Altogether, with the lantern, level, &c., it would be about £10. Without some of these accessories, £8. It was supposed to stand on a brick pier in the garden, the supports being covered over when the instrument was not in use, whilst the telescope could be taken up and carried into the house. From the point of view of astronomers who work to the 100th of a second it would be, of course, a rough instrument, but its simplicity and cheapness were its great recommendations.

Mr. De La Rue. Is it much used by country gentlemen? (Laughter.)

Mr. Lecky said he did not know anything about the sales; but coming from *Mr. Latimer Clark*, he thought the instrument worth bringing before the Society.

A Fellow. Does not the reflector cut off some of the object-glass?

Mr. Lecky. Yes; the reflector is certainly rather wide, and it might well have been made smaller.

Mr. Rand Capron. It seems to me the lantern is one of the points that look rather objectionable; but the difficulty might be got over by substituting one of the new incandescent lamps. With a small transit instrument, a good deal larger than this, and with a hollow axis, I have been trying a $2\frac{1}{2}$ -candle power incandescent

lamp, and that seems to me to have a real advantage over all candle-lamps or oil-lamps. You have more light, there is no heat, and the field is nicely illuminated. I find the bichromate batteries are always liable to drop in power, and when newly excited they are apt to destroy the carbons. I am using a permanent form of battery which is always in use in my observatory, and which gives a sufficiently powerful current for me to use it to light up a Geissler-tube for comparison with comet spectra and similar purposes. I am inclined to think that these lamps will soon supersede most of the forms of lamp which have been generally used in the past.

The President. In what manner do you work the battery you speak of?

Mr. Rand Capron. It is, I think, what is called the Granville battery. It is composed of a zinc rod in a porous cell, the cell having a small quantity of mercury put into the bottom in order to keep the zinc amalgamated. This is excited by a small quantity of diluted sulphuric acid. The outside cell contains a plate of carbon immersed in a solution of bichromate and muriatic acid. This is very permanent and will last for six months, at the end of which time it gets a little out of order; but at the bottom of the vessel a hole is purposely left in which a cork is inserted, and by drawing out the cork the solution runs out, and can be replaced without any trouble.

The President. I am afraid this instrument will not be used much by country gentlemen. The time can be got from Greenwich with such facility that I do not think they will care to use this instrument. Some such small instrument as this will be useful in the colonies; and if you can determine the time within 1 or 2 seconds, it will be an accuracy not generally attained there.

Mr. Knobel. Do not you think the dipheidroscope is sufficient for all practical purposes? I can determine time by it to a second, and it can be used very readily.

Mr. Lecky. From the sun alone of course.

Mr. Rand Capron. The lamps I speak of are excited by 4 cells, 8 in. by 6 in. external size. The whole cost is about 28s., lamp and cells included.

The President. I am afraid for colonial use the batteries would be out of the question.

Mr. Marth wished to refer to a subject which had been mentioned earlier in the evening. The reason of the apparent great error in the prediction of the eclipse of Jupiter's fourth satellite was this. For two years or two years and a half there had been no eclipses of this satellite, and the eclipse of April 4 was the second of a new series. The satellite had passed outside the shadow of Jupiter for a number of revolutions, but had been gradually approaching it, until at last on March 18 it just passed through the extreme edge of the shadow, taking 58 minutes to describe the chord. In the second eclipse, that of April 4, it took 86 minutes; in the next

eclipse, April 21, it would take $1^h 47^m$; and the duration of the eclipses would constantly increase until September 4 in next year, when the duration would be $4^h 47^m$. If in that case there should be an error of more than a minute or so in the time of disappearance, it would be a grave reproach to the tables. Near the central eclipse the times of duration did not vary much, but just at the beginning or end of a series they were very variable. The whole theory of Jupiter's satellites required a thoroughly new settlement. Damoiseau's tables were based upon the early observations of the eclipses, but the positions of the nodes cannot be satisfactorily determined from eclipses. But if observers would watch the transits of the shadows of the satellites, and note when they were just in the centre of the disk in latitude, that observation would be of the greatest service, and he hoped it would be made in the autumn. It seemed to him that Capt. Noble was a little hard on the 'Nautical Almanac' predictions; the difference of 24 minutes did not imply any great error in the tables after all, since the slightest difference in latitude made an enormous difference in the duration of so early an eclipse.

The President. Yesterday I brought before the Royal Society some work upon which I have been engaged for some time; and perhaps it may be interesting to the Society that I should briefly indicate the nature of the work. I suppose it is known to all the Fellows of the Society that of late years there have been indications of something having gone fundamentally wrong with our theories, that there has been a large apparent discordance between the theoretical acceleration of the Moon's motion as determined by Professor Adams and that apparently required to reconcile Hansen's Tables with the observations; but, independently of that, there have been distinct indications of late of a most extraordinary change in Hansen's tables; and the error has rapidly increased within the last few years, and has now become so large that there can be no doubt whatever about its reality; it has separated itself entirely from all errors which might arise from systematic errors of observation, and demands an explanation. Several astronomers have therefore given great attention to the subject, and have looked for the explanation in the lunar theory, expecting that there might be some long inequality which had not been taken account of hitherto. I have also given the subject great attention for some considerable time; and though working on my own lines, I also, at first, sought the explanation in the lunar theory itself. But after working at the lunar theory for some time, I found no indication of anything likely to lead to the quantity I was seeking for; and I made up my mind that the effect on the lunar theory must be indirect rather than direct, and, as an error in the solar theory might be multiplied about thirteen times in the lunar theory, it appeared to me more likely that the quantity I was looking for was in the solar theory rather than in the lunar theory directly. The solar theory, however, had

been carefully revised of late years by Hansen in his Tables of 1853, and by Le Verrier in his Tables of 1859. These two tables are, comparatively speaking, accordant; and this agreement appeared to render a search for long inequalities not very promising. In this way I gradually concentrated my attention upon the mean longitude of the Sun, thinking, if anywhere, that was the place where the error would most likely come in. What I have found is, I think, decisive upon the point—that the cause lies there. For a very great number of years, in fact, either directly or indirectly, all the observations from 1750 down to 1863 have been reduced by means of the expression for the mean longitude of the Sun (or what I should rather call the longitude of the mean Sun) from Bessel's formula, whilst in more recent years astronomers have been using either Hansen's or Le Verrier's expression. If you reduce these expressions down to 1850, which I have done for comparison with the others, we have the following expressions of the longitude of the mean Sun:—

1850 + t , Jan. 1, Paris mean noon.

$$B = 280^\circ 46' 36'' \cdot 12 + 1296027 \cdot 618184 t + 0 \cdot 0001221805 t^2$$

$$H = 280^\circ 46' 43'' \cdot 20 + 1296027 \cdot 674055 t + 0 \cdot 0001106850 t^2$$

$$L = 280^\circ 46' 43'' \cdot 51 + 1296027 \cdot 678400 t + 0 \cdot 0001107300 t^2$$

In all these expressions the unit of time has been *supposed* to be a Julian year of 365·25 mean solar days.

The constant differences 7"·08 and 7"·39 in $B-H$ and $B-L$ are not unimportant, for they introduce abrupt changes in the record of time; but the differences in the coefficients of t and t^2 show that the same unit of time cannot have been adopted in these expressions. The measure of time must be continuous; let, therefore, 1 and $(1+x)$ be the units of time really adopted in B and H ; then

$$\begin{aligned} & 1296027 \cdot 618184 t + 0 \cdot 0001221805 t^2 \\ &= 1296027 \cdot 674055 t(1+x) + 0 \cdot 0001106850 t^2(1+x)^2. \end{aligned}$$

If, therefore,

$$\begin{aligned} n &= 1296027 \cdot 674055 \\ x &= -\frac{0 \cdot 055871}{n} + \frac{0 \cdot 000114955 \cdot t}{n}. \end{aligned}$$

To reconcile B and H therefore, x cannot even be a constant. Similar remarks apply to the differences between B and L .

Now let N be the Moon's mean motion referred to 1 as a unit of time; $N + \delta N$ the Moon's mean motion referred to $(1+x)$.

Then $(N + \delta N)(1+x) = N$

$$\text{and} \quad t \cdot dN = \frac{N}{n} [0 \cdot 055871 \cdot t - 0 \cdot 0000114955 t^2]$$

$$= 0 \cdot 747 t - 1'' \cdot 54 \left(\frac{t}{100} \right)^2.$$

But Hansen determined his mean motion of the Moon so as to force an agreement between his theory with unit $(1+x)$ and observations reduced with unit 1. Directly therefore the unit of

time was consistently changed to $(1+x)$, the agreement between his theory and observation ceased.

Now Le Verrier's Solar Tables were adopted in the 'Nautical Almanac' for 1864, and with them his unit of time, differing but slightly from Hansen's. The consequence has been that in the years 1864 to 1880 the error of Hansen's longitudes has increased from $+0''.121$ to $10''.265$, whilst no progressive change is shown from the observations 1847-1863.

The errors of the Solar Tables have also, from the same cause, changed from $+0''.025$ in 1864 to $+0''.861$ in 1880.

The term $-1''.54 \left(\frac{t}{100}\right)^2$ would indicate that Hansen's Tables should require a larger value of the secular acceleration to represent observations than would have been the case had not this confusion of units taken place.

If, as some recent discussions have apparently indicated, Hansen's Tables require a value of about $8''.8$ of secular acceleration, this would reduce the value to $7''.3$, whilst Adams' and Delaunay's theoretical value is about $6''.1$; but too much stress must not be laid upon these agreements or disagreements.

If I am right in this matter, Hansen's Tables are unfortunately not to be trusted in the discussion of ancient Eclipses until the effects of this confusion of units of time have been cleared. The effects of this error are not, however, confined to the Lunar Tables, although, from the rapid motion of the Moon, they are there presented in a magnified form. This unconscious change of units of time is not a new thing. It has been going on for years. Every new long inequality in the Sun's motion which has been brought to light, new determinations in the constants connected with the precession of the equinoxes, and changes of obliquity have indirectly been treated so as to change the unit of time; and as the effects of these changes have not been consistently carried into all the constants multiplied by t , confusion and error have resulted.

The course to be adopted in reconstruction appears to me that some definite expression should be adopted as the longitude of the mean Sun, and that this should remain unchanged; and that if L_0 be this expression, then all changes which the progress of science should indicate as necessary should be thrown upon δL ; so that the mean longitude of the true sun $= L_0 + \delta L$. When this has been done and carried consistently through our theories, we may be in a position to discuss with some certainty whether there is or is not change in the time of the Earth's rotation on its axis. At present I think that we are not in that position. Of course this is a serious statement to make; but I can see no escape from the conclusions drawn.

[*Note.*—When the difference between the longitude of the mean Sun and the mean longitude of the Sun is strictly investigated, there appears a secular term $-0.3113 \left(\frac{t}{100}\right)^2$ in the correction δL , the

neglect of this difference in computing the mean sidereal time at noon would give rise to more than 4" in the apparent secular acceleration of the Moon's mean motion required to reconcile the theory and observation. This result has been communicated to the Royal Society.—E. J. S.]

Mr. Hind. I am sure all the Fellows of the Society must have heard with the greatest interest the outline of this paper which the President has taken the trouble to give us. I believe he has communicated it to the Royal Society in greater detail.

The Earl of Crawford and Balcarres. May I ask whether the President would make an abstract of the paper for our 'Monthly Notices' as well as for the Royal Society?

The President. Perhaps I may be able to draw a slight draft of it. It is rather difficult to give it apart from the explanation. I think I have given the essential point, which depends upon the break of continuity that has taken place in the measure of time. It has been done unconsciously, because we have been seeking to improve the theory, and while we have been so doing these changes have been occurring gradually.

Mr. Hind drew attention to a circular received from the Earl of Crawford's observatory, announcing the discovery of D'Arrest's comet. It was now announced that the object seen was not a comet, but a new nebula. In the first circular, however, not only was the place of the object given, but its daily motion. He would like to know how this had been determined, seeing that the object was now found to be a nebula, which of course did not move*. (Laughter.)

The following papers were also announced :—

Dr. de Konkoly. "Spectroscopic Observations of Comet Swift, 1883."

J. Tatlock. "On the Position of λ Ursæ Minoris."

E. E. Markwick. "Notes on the Great Comet (b) 1882."

D. Gill. "Note on some Criticisms made by Mr. Stone on the Methods available for determining the Solar Parallax."

J. Morrison. "On the Computation of the Eccentric Anomaly, Equation of the Centre and Radius Vector of a Planet, in terms of the mean Anomaly and Eccentricity."

Prof. A. Hall. "Observations made at the U.S. Naval Observatory, Washington."

Astronomer Royal. "Observations of Comet *a* 1883 made at the Royal Observatory, Greenwich."

The following gentlemen were duly elected Fellows of the Society :—H. J. Chaney, Rev. T. Harley, B. J. Hopkins.

The Meeting adjourned at 9^h 30^m.

* Prof. Krueger has since explained how this happened. See p. 163.

Professor Newcomb on the Present State of the Theories of the Celestial Motions.

SEVERAL valuable papers on subjects of the highest astronomical importance have been published from time to time during the last two or three years by the American Nautical Almanac Office. In connection with the sixth of these—a “Discussion of Observed Transits of Mercury, 1677–1881,”—with which the first volume of the series is completed, Prof. Newcomb has prepared an Introduction explaining the objects of the work of which this volume is the commencement. These objects are a systematic determination of the constants of astronomy from the best existing data, a re-investigation of the theories of the celestial motions, and the preparation of tables, formulæ, and precepts for the construction of ephemerides and for other applications of the results. In explaining the origin and purpose of the great undertaking which he has thus inaugurated, Prof. Newcomb has given so able an account of the present state of our theories of the celestial motions that the reproduction of it, in a very slightly condensed form, in the pages of the ‘Observatory’ will probably prove interesting to all its readers.

Prof. Newcomb commences by referring to the increasing discordance between theory and observation as to the places of the moon and planets. Not, indeed, that, so far as the current requirements of astronomy are concerned, this deviation has yet reached a serious amount. “Not only may the planets be found and eclipses predicted for many years to come by the present tables, but, with the exception of the Moon, there is every reason to suppose that the tabular positions will serve the purposes for which they are immediately required in navigation and practical astronomy. But when we take a wider view and consider the general wants of science both now and in the future, we find that in the increasing discordance between theory and observation there is a field which greatly needs to be investigated.

“If mutual gravitation according to the law of Newton is the only cause which changes the motions of the planets, then it is mathematically possible to construct tables which shall represent observations with the last degree of precision and through any period of time. It is quite possible that the discordances alluded to proceed solely from the imperfections in the mathematical theory, and do not indicate any unknown cause affecting the celestial motions. But when we investigate more closely, and seek to ascertain the cause of such discordances, we find a state of things which renders it impossible to draw any definite conclusion respecting the ultimate possibility of representing observations by existing physical and mathematical theories. This state of things has its origin in the comparative brevity of the period during which accurate observations have been made, and in the difficulty of conducting, on a systematical plan, mathematical investigations having in view the perfection of astronomy.”

But the element of time is one which enters very greatly into the requirements of astronomy. "Since astronomical generalizations rest, not upon experiments, but upon observations, it is always necessary to wait for the recurrence of the phenomena on which the conclusions are to depend. The main object of investigation being the forces which change the motions of the planets, we must observe these motions during a sufficient period to make evident the action of the forces. The longer the time which elapses the more material we have for reaching conclusive results. It is generally considered that accurate observations commenced with Bradley in the middle of the last century. The period during which they have continued is therefore about a century and a third. But there are many exceptions in the case of different classes of observations. The places of the Moon have been traced backward with a nearly modern precision through the century preceding Bradley's observations, while the observations of the Babylonians and the Arabs are still of the greatest value in the lunar theory. On the other hand, none of Bradley's instruments fulfil the requirements of the present time, and his observations were in many cases extremely defective as compared with our own. If, therefore, we attempt to learn what conclusions can be reached in the present state of astronomy, we must consider each object of observation separately with reference to its general place in a comprehensive scheme.

"But time is not the only element which comes in. If we are to determine what unknown causes affect the motions of the planets the first step is to prove that there is really a discordance between the results of observations and the results of the theory of gravitation. The first step towards establishing such a discordance is the construction of tables and formulæ of which we can say that they are beyond reasonable doubt the results and the only results of the gravitation of the known bodies of the solar system. The necessary conditions which such tables and formulæ must satisfy are that they shall be founded upon uniform elements and data, and that the results of employing the adopted elements shall be carried out with all necessary precision. Now, not only has this requirement never been fulfilled, but the effect of recent advances in exact astronomy has rather been to carry us away from its fulfilment.

"It is scarcely possible for a year to pass without some new investigation, or series of observations, which shall materially add to the precision with which we can determine some astronomical constant. Each astronomer who finds material to be used in this way, is naturally desirous of utilizing it to its fullest extent, and is therefore under a temptation to introduce each new improvement into his investigations without respect to their consistency with the investigations of others which have been made with the older data. Sometimes, too, the object of constructing an astronomical formula is to correct it from time to time, and the very object of the constructor may tend to destroy its consistency. A brief glance at

some features of the existing planetary tables will illustrate the point in question.

"Laplace, in the third volume of his '*Mécanique Celeste*,' constructed by the most rigorous and complete methods then known to science, a complete theory of the planetary perturbations, founded on elements and masses which are quoted in Chapter VI. of his work. From his results tables were constructed by Lindenau and Bouvard during the early years of the present century.

"In order to give the tables the required precision it was necessary to correct the elements by a comparison with observation. Thus, the new tables no longer correspond to the original formulæ of Laplace. Moreover, the theory was in many respects so imperfect that no certain conclusion could be drawn from a comparison with observation. This was notably the case with the perturbations of the second order. It was therefore necessary to make a complete reconstruction of the theory. Nevertheless, such was the labour and difficulty of constructing new tables that those of Lindenau and Bouvard remained the standards for use in the preparation of ephemerides during nearly half a century.

"The next complete reconstruction of the theories and tables of the planetary motions was that of Leverrier. His work on this subject forms the most important part of the fourteen volumes which he published under the title '*Annales de l'Observatoire de Paris*.' The first of these volumes appeared in 1855, the last in 1877.

"Some considerations of the circumstances under which this great work was carried out, and of the objects at which it aimed, may not be out of place as showing how it happens that more remains to be done in the same direction. When Leverrier commenced his work, the most striking feature which presented itself was the imperfections of the tables of Lindenau and Bouvard. The formulæ on which they were constructed, though fully up to the science of the time in which they were formed, were far behind modern requirements in generality and rigour. Better tables and formulæ constituted one of the most pressing wants of exact astronomy. Both his position and his previous works marked Leverrier as the one to undertake the work of constructing such tables and formulæ. Naturally desirous of beginning to reap the results of his labour as soon as possible, he investigated the elements of the planets and published the corresponding tables one or two at a time. This course did not detract from his main object, that of constructing improved planetary tables. But there was another object, the desirableness of which was not immediately felt, but which must be more and more felt in the not distant future, namely, the attainment of uniformity in adopted astronomical data. So far was Leverrier from aiming at this object, in its entirety, that his tables do not, in all cases, embody his final results. The consequence is, that notwithstanding that his work makes a greater epoch in astronomy than any of his immediate successors can hope to make, it does not wholly supply the wants

of science in the immediate future. In many of his tables large and increasing deviations from observation already exhibit themselves. This is most notably the case with the planet Saturn, the theory of which he did not succeed in bringing to a satisfactory conclusion. The geocentric places of Mars and Venus are also largely in error at the time of the nearest approach to the Earth. The earlier tables, those of the Sun and Mercury, are the only ones which can be regarded as entirely satisfactory in their agreement with observations, with the possible exception of Uranus and Neptune."

Prof. Newcomb adds that what he has said of Leverrier's tables applies with greater force to his own tables of Uranus and Neptune. "Their main object was to supply an immediate astronomical want. The data on which they were found could not be regarded in any respect as definitive, nor were the adopted masses absolutely uniform. The formulæ of perturbations on which they depend are also such that we cannot say with certainty whether the deviations from observations which they exhibit arise from any other cause than the imperfections of the theories on which they are founded.

"Now, the material available for the accurate determinations of the fundamental elements of astronomy has increased many fold since the conclusion of Leverrier's work on the four inner planets. The recurrence of transits of Venus and Mercury, the perfection of astronomical instruments, the employment of improved places of the fixed stars, the introduction of more systematic methods of research, and the reinvestigation of older observations have all combined to bring precise astronomy to a higher plane than it ever before occupied. Supposing that their mutual gravitation is really the only cause which disturbs the elliptic motion of the planets around the Sun, it is now theoretically possible to construct tables of all the large planets except Neptune, from exact data, which shall represent observations within their probable errors until the middle of the next century. The desirableness of having such tables founded on one consistent and fully elaborated theory hardly needs to be insisted on. Only in this way can it be decided whether deviations from theory arise from its imperfections, or from the action of unknown and, perhaps, unsuspected causes."

Another and an urgent reason for action lies in the great accumulation of observations, which, though made by eminent astronomers and with the best appliances, have never appeared in any of the existing tables, and are in danger of going to utter waste. And this mass of unused material increases so rapidly "that it is even now entirely beyond the power of individual management, and the longer it is left the less hope there is that it ever will be managed. The required work must be that of an organization rather than that of an individual," and should be founded not only on all the observations which it is practicable to employ as its basis, but should utilize, so far as possible, all discussions by other astronomers.

On assuming the superintendency of the American Ephemeris in 1877, Prof. Newcomb determined to employ the resources at his disposal to carry out, or at least to enter upon, the work in question; and now, although what has yet been done is only a commencement, the prospects of being able to carry it through are fairly good. Both Congress and the Navy Department have supplied all the assistance which has been asked for; and a force of from eight to twelve computers, some of the highest order of mathematical ability, has been actually employed for some time past, and may, if necessary, be increased in the future. Under these circumstances Prof. Newcomb has felt himself warranted in announcing his programme, the rather that by his so doing much duplication of work may be avoided, and he presents the following statement of the unpublished work now in hand and general programme for its continuance.

"The theories of the four inner planets naturally claim the first attention as embodying most of the fundamental elements of astronomy. This branch of the work includes not only the masses of the planets and the elements of the respective orbits, but the constants connected with the rotation of the Earth on its axis, namely, the annual precession, the obliquity of the ecliptic and its secular variation, the position of the equinox among the stars, and, indirectly, the positions of the fundamental stars. To these may be added the solar parallax and the mass of the Moon, as well as a number of quantities connected with those already mentioned. In the determination of these constants the plan, as already mentioned, contemplates the utilization and combination of all valuable data.

"Besides what is found on the general subject in the present volume, the following works are finished or are in progress:—

"Leverrier's tables of the Sun, Mercury, Venus, and Mars have been partially reconstructed with a view of making them more convenient in use. His theory, however, remains unaltered in the manuscript tables. A comparison of the Greenwich, Paris, and Washington meridian observations of Mercury with those tables has been commenced and is approaching completion. Similar comparisons for the Sun, Venus, and Mars have not been seriously commenced, but it is expected to commence them in the course of the year 1883.

"A discussion of the corrections required by the older Greenwich observations up to 1830, as published by Prof. Airy, in order to reduce the results to a uniform system, is nearly completed, and is expected to appear as Volume 2, Part I. of these papers.

"General tables and formulæ for forming the differential coefficients for correcting the elements of the inner planets have been prepared, and it is intended to publish them in the next volume.

"Although the final completion of the theories of the other planets must follow the work on the interior planets, it is advisable to begin it without delay, owing to the great labour which it involves. The general perturbations of Jupiter and Saturn were therefore taken up by Mr. George W. Hill in 1877, but they are still unfinished. It is now expected that Mr. Hill's work will be

completed about the end of 1883. The computation has been made principally by the methods of Hansen.

"Much attention has also been paid to the subject of the Moon's motion. The first object has been the continuance of the discussion of eclipses and occultations previous to 1750 up to the present time. The reason for laying so much stress upon occultations is that notwithstanding the irregularity with which they are observed, their considerable accidental errors and the labour of reducing them, they constitute the only observations of the Moon which are free from systematic error, and which can therefore be used with safety to compare the mean longitudes of the Moon at wide intervals of time.

"Tabular positions of the Moon, as well as those of the fixed stars, are now complete for all the more important occultations since 1750, and the reductions for parallax are in progress. Should the work not be intentionally delayed in order to bring it up to the date at which new tables of the Moon shall be actually constructed, it may be expected that this particular discussion will be terminated by the end of 1884."

All the observed and recorded eclipses of Jupiter's satellites have been computed from Damoiseau's tables up to the early part of the present century; but this work, which was taken up on account of the light which the motions of the first satellite may throw upon the question of the uniformity of the Earth's rotation, has been discontinued for the present.

"An essential and very laborious and very difficult part of the work is that of preparing formulæ and tables for computing the general perturbations of all the planets. A problem which has taxed the powers of the greatest mathematicians of modern times, and the solution of which is still, after all their work, in an unsatisfactory state, is one which the writer feels most hesitation in approaching. He has, however, devised a method which he hopes may prove convenient in practice for the general development of the disturbing function and its derivatives. Whether any improvements can be devised in the method of integrating must be left to the future."

"In the future work it is intended to combine the data in a way different from that generally adopted. When all four of the inner planets are considered together it is possible greatly to strengthen the results on special points. An example of this is afforded by the relation of observations on Mercury and Venus to the obliquity of the ecliptic and the position of the equinox. Hitherto these quantities have been made to depend solely upon observations of the Sun;" but the conditions under which these are made renders it impossible to view them without a strong suspicion of their being affected by systematic errors. It is possible, however, from meridian observations of Mercury alone (and nearly the same remarks apply to those of Venus, and to a limited extent to those of Mars), "to obtain the principal elements of the Earth's orbit round the Sun, including the absolute longitude of the Sun itself, and hence a separate determination for the position of

the equinox. It is true that some of the elements, especially the eccentricity and longitude of the perihelion, may prove to have small weight; but this is because what is most accurately given by the observations will be a linear function of the corrections to these elements. But even such a result will furnish valuable data for the final values of the necessary quantities."

"In theory, observations of the Moon might also be utilized for an absolutely independent determination of the equinox and of the obliquity of the ecliptic. But the rapid motion of the Moon in declination when near either equinox introduces a large probable systematic error into the measures made upon it at any definite moment. No weight can therefore properly be assigned to a position of the equinox by meridian observations of the Moon. The obliquity derived from such observations may, however, be worthy of more consideration.

"The position of the Sun among the stars may, however, be determined through the aid of the Moon with a considerable approach to precision. By observations of eclipses, especially at the beginning and ending of totality, the exact moment when the Sun and Moon are in conjunction is determined with great precision. By observations of occultations the mean position of the Moon among the stars is determined with yet greater precision. Hence, by a combination of the two, we have a result for the position of the Sun among the stars which may possibly be entitled to considerable weight. It is, however, a drawback to the method that few observations of eclipses having any claim to precision were made between 1720 and 1800, while those made before 1720 are of course subject to more or less suspicion of systematic error.

"It is worthy of note that this method of determining the position of the Sun among the stars is, in principle, that adopted by Hipparchus and Ptolemy.

"The above are the leading features in which the plan of the proposed work differs from that hitherto followed. The objects are also somewhat different, in that they include a basis for future conclusions as well as the determination of astronomical constants and the construction of new tables. It is hoped, should the work be completed on the proposed plan, that for a miscellaneous and frequently inconsistent combination of astronomical constants there will be substituted a consistent set, and that the result of this substitution will be to make it easy to determine, from any future deviation between theory and observation which may show itself, in what direction we are to look for the cause."

Astronomical Photography.

THE important part that photography is likely to play in the future of astronomy renders it desirable that an opportunity should be afforded to astronomers to acquaint themselves with the improvements continually made in this branch of their science. This could best be done by the establishment at convenient places of

collections designed to exhibit the progress of photography as applied to astronomical observations.

The Harvard College Observatory has some special advantages for forming such a collection, since it already possesses many of the early and historically important specimens which would naturally form part of the series. Among these may be mentioned four series of daguerreotypes and photographs of various celestial objects taken at this Observatory. These series were respectively undertaken in 1850, 1857, 1869, and 1882.

At present, the astronomers of the United States have no ready means of comparing their own photographic work with that done in Europe, or even with that of their own countrymen. The proposed collection of photographs, so far as it could be rendered complete, would greatly reduce the difficulty.

It is therefore desired to form, at the Harvard College Observatory, a collection of all photographs of the heavenly bodies and of their spectra which can be obtained for the purpose; and it is hoped that both European and American astronomers will contribute specimens to this collection. Original negatives would be particularly valuable. It may happen that some such negatives, having slight imperfections which would limit their value for purposes of engraving, could be spared for a collection, and would be as important (considered as astronomical observations) as others photographically more perfect. In some cases, astronomers may be willing to deposit negatives taken for a special purpose, and no longer required for study, in a collection where they would retain a permanent value as parts of an historical series. Where photography is regularly employed in a continuous series of observations it is obvious that specimen negatives only can be spared for a collection. But in such cases it is hoped that some duplicates may be available, and that occasional negatives may hereafter be taken for the purpose of being added to the collection, to exhibit recent improvements or striking phenomena.

When negatives cannot be furnished, glass positives, taken, if possible, by direct printing, would be very useful. If these also are not procurable, photographic prints or engravings would be desirable.

In connection with the photographs themselves, copies of memoirs or communications relating to the specimens sent, or to the general subject of astronomical photography, would form an interesting supplement to the collection. A part of the contemplated scheme will involve the preparation of a complete bibliography of the subject, including a list of unpublished photographs not hitherto mentioned in works to which reference may be made.

The expense which may be incurred by contributors to the collection in the preparation and transmission of specimens will be gladly repaid by the Harvard College Observatory when desired.

Cambridge, Mass.,
1883, Feb. 21.

EDWARD C. PICKERING,
Director of the Harvard College Observatory.

CORRESPONDENCE.

*To the Editor of 'The Observatory.'**The Star of 1572.*

SIR,—

Allow me to address you a few words more on this subject, if only to excuse my forgetfulness that the stars in the vicinity of Tycho Brahe's star (called by Bayer B Cassiopeiæ) have been examined in recent years, particularly by Mr. Hind and Mr. W. E. Plummer at Mr. Bishop's Observatory, Twickenham, about ten years ago. They found that the star observed by D'Arrest and numbered 129 in his Catalogue* (with map) of 212 stars in a region around it embracing 4^m of R.A. and 1^o of N.P.D., is really subject to a fluctuation of light, though the extreme amount of change "scarcely includes a whole magnitude" (Monthly Notices, vol. xxxiv. p. 168). The place of this star for 1865, as determined by D'Arrest, is R.A. $0^h 17^m 18^s$, N.P.D. $26^o 37'$, which, reduced to the present year, would be R.A. $0^h 18^m 16^s$, N.P.D. $26^o 31'$. It appears to me that further observations not only of this, but of other stars in the neighbourhood of B Cassiopeiæ are desirable, especially as Tycho did not state the times of his observations. And in this connection I would invite attention to the suggestions in Prof. Pickering's "Plan for Securing Observations of the Variable Stars," which you have published recently.

I have examined Edward Pigott's paper on "those stars which the Astronomers of the last Century suspected to be changeable," read before the Royal Society on February 9th, 1786. He says that believing in the probability of the conjecture of Keill and others that the famous star observed by Tycho Brahe in 1572 would return periodically about every 150 years, and that though it had not been noticed at the end of the first period in question, this might be because the brightness was not so great at each return, he took a plan of the stars near its place in September 1782, and repeatedly examined them afterwards without being able to perceive indications of change in the magnitude of any one of them.

We are informed by Wolf that Leowitz published in 1573 a tractate (both in Latin and German)† entitled "Judicium de novâ Stellâ." This work must be very scarce, for it is not even mentioned, in Poggendorff's 'Biographisch-Literarisches Handwörterbuch,' where the other works of Leowitz (who died in 1574) are enumerated. I do not think that a single copy of it exists in this country; but this is of no great importance, as he seems to have been chiefly a hunter of astronomical facts for their supposed astrological significance, and his own opinion is not entitled to

* Oversigt over det Kgl. danske Videnskabernes Selskabs for 1864.

† Wolf, 'Geschichte der Astronomie,' p. 415, note.

much weight. Tycho was evidently acquainted with all that Leowitz had written on the subject; and his view, mentioned in my last letter, is doubtless the correct one. He gives a full quotation from Leowitz, who, after referring to the supposed star seen in the reign of the Emperor Otto I. (anno 945), and the calamities that happened at the time of its appearance ("propter peregrinas gentes infusas in Germaniam"), adds, "Verum, multo locupletius testimonium in historiis extat de Anno Domini 1264, quo Stella magna et lucida in parte Coeli Septentrionali circa Sydus Cassiopeæ apparuit, carens similiter crinibus, ac destituta motu suo proprio," and then adduces historical troubles of that time, omitting, however (I suppose from not being an Englishman), the battle of Lewes and the capture of Henry III. and his son Prince Edward by the rebellious barons. It appears to me that both in 945 and 1264 the comets of those years are in fact referred to, the splendid comet of 1264 (which, being thought to have been identical with that of 1556, was expected again about twenty-five years ago, but failed to put in an appearance) having been perhaps observed at one time when nearly stationary and when its tail was not conspicuous.

In conclusion, let me note that at line 12 of my former letter the word "repeated" is printed by mistake for "reported."

Yours faithfully,

W. T. LYNN.

Blackheath, 1883, April 4.

P.S.—My attention has been called to a letter by Mr. Sadler (signed H. S.) in the 'English Mechanic' for 1880, January 2, in which he states with great clearness the facts about this star, and also gives a small map to indicate its place, as a guide to the amateur. I should also remark that I am indebted to Dr. J. L. E. Dreyer, of Armagh, for pointing out that there is an error in the difference (as given by me last month) between the right ascension deduced by Argelander from Tycho's observations of the Nova of 1572 (sometimes called the Pilgrim) and that of the star observed by D'Arrest, forming No. 129 in his catalogue. It should be half a minute of *arc*, not of *time*. D'Arrest's place is R.A. $0^h 17^m 18^s$, $4^\circ 19' 30''$, N.P.D. $26^\circ 37' 1''$.

The Reduction of Transits.

SIR,—

In your Number for this month there is a letter from Mr. Hunt on the Reduction of Transits. I think there is an easier and more general way than his, which only requires the slide rule. I know you use slide rules for many stars at Greenwich; but the plan has not come into general use, because it entails the calculation of constants depending on the declination and latitude; but if we compute Bessel's m and n ,

$$\begin{aligned} m &= b \cos \phi + a \sin \phi, \\ n &= b \sin \phi - a \cos \phi, \end{aligned}$$

then the correction is $m+n \tan \delta + c \sec \delta$, and the whole of the necessary computations can be made easily by the slide rule for any star.

The great advantage of a slide rule is that you see the accuracy you want at once and are free from some sources of error. Of course the rapid change of $\tan \delta$ near the pole makes this plan useless there, but stars for which the slide rule is useless are also of no value as time stars.

Those who are disposed to try the slide rule for numerical computations will find it worth while to try Professor Fuller's slide rules, made by Stanley of Great Turnstile. I have had one in frequent use for some three years now in this trying climate, and can strongly recommend it. The state of things has changed since the passage Mr. Hunt quotes from De Morgan was written. Fuller's rule is very nearly equivalent to five figure logarithms, and greatly facilitates interpolations, especially where many are required. I send you by book-post a Table of coin values which has been entirely computed by its aid from the weights and finenesses given in the units in use in the countries named; it will give some notion of the power of the instrument.

I am, yours truly,

J. F. TENNANT.

H.M.'s Mint, Calcutta,
1883, Feb. 24.

The Mode of Formation of Lunar Craters.

DEAR SIR,—

In No. 70 of the 'Observatory' is inserted a communication of Herr Bergeron on the origin of cavities on the surface of dough-like masses, and from the similarity of these cavities to lunar craters inferences are drawn with regard to the similar nature of their production.

I have the honour herewith to lay before you a representation of the results of my experiments, in which may be recognized not similarity merely, but almost complete identity with the smallest details of the Moon's surface, and that not in regard to the so-called craters alone. The mode of procedure is accurately described in 'Sirius' for 1882, p. 59, and the results have been obtained by me through a process arrived at step by step in the explanation of meteorites.

Should you wish to make use of this communication, it will be easy for you to produce some new impression with the materials referred to, and then have these photographed and printed. Perhaps the experiment will give occasion for an even closer investigation of the origin of the action than has hitherto been obtained.

Yours faithfully,

A. MEYDENBAUER,
University Architect.

Marburg, Prussia,
1883, April 7.

The Taurids.

SIR,—

This shower appears to have returned with some richness last November, and to have supplied, as usual, a large proportion of fine meteors. A valuable list of observations made at the Stonyhurst Observatory has been obligingly furnished me by the Rev. S. J. Perry; and of 36 meteors seen during the first half of November, 13 were Taurids with a radiant at about $\alpha 60^\circ$, $\delta 21^\circ+$. There were also distinct traces of radiation from Camelopardus, Perseus, and Ursa.

Two brilliant Taurids were seen at Bristol on Nov. 6, $16^h 36^m$, and Nov. 11, $13^h 44^m$; they exhibited the usual features, moving rather slowly and throwing off trains of sparks.

Yours faithfully,

W. F. DENNING.

Bristol, 1883, April 4.

19 Leporis.

SIR,—

On the evening of the 12th instant, I compared the Great Comet δ with two stars, one of the 7 and the other of the $8\frac{1}{2}$ magnitude. The brighter star, which was rather red, I have now identified as 19 Leporis. I find the following magnitudes assigned to it by different catalogues:—Lalande gives 6, Argelander-Oeltzen 5 and 6, the 'Uranometria Argentina' 5.9 , and finally Stone's Cape Catalogue, 1880, 4. Dr. Gould records the star as red, but does not hint at its variability. If the Cape record is neither a misprint nor an overestimate of magnitude, it is quite clear that the star is a variable within pretty wide limits. Perhaps some of your contributors that are posted up in the department of variable stars can say if this star is regarded either as a known or as a suspected variable. I have consulted many authorities, but cannot find that it is regarded in either character.

I am, Sir, yours faithfully,

JOHN TEBBUTT.

Windsor, N. S. Wales,
1883, February 16.

NOTES.

MEASURES OF THE RINGS OF SATURN*.—In a paper in Astr. Nach. No. 2498, M. Struve remarks that, induced by the fact that Saturn was last year in the same position with respect to the Sun and the Earth as when he measured the dimensions of the rings in 1851, and also invited by the persistently favourable weather of last summer, he executed a new series of measures of the rings. In the paper published by him about the end of 1851, "Sur les

* Astr. Nach. No. 2498

dimensions des Anneaux de Saturne," founded chiefly on the measures of that year, he endeavoured to prove that the measures and drawings of the rings made since the time of Huygens indicate considerable changes in the system, and that these changes might be very satisfactorily explained by supposing that, whilst the exterior diameter has remained constant, the ring itself has been continually widening towards the interior so as to approach nearer and nearer to the body of the planet. He believed, after allowing for a plausible assumption concerning the irradiation which is known to have greatly affected the oldest measures and estimates, that the annual contraction of the interior ring towards the planet might be put at $0''.013$. Supposing this, then, to have been the case, the approach in question would amount, in the 31 years which last year had elapsed since 1851, to $0''.4$, a quantity which would be most distinctly shown in a fresh set of measures. And this could be done with so much the more certainty for the purpose of comparison that the measures would be made by one and the same observer, and using the same instrument, so that nearly all the circumstances would be precisely the same. However, although the observations of last year were made in general when the images were good, M. Struve's impression is that in this respect the conditions were more favourable in 1851.

Only six complete sets of measures were obtained, between August 17 and September 17, of the distances of the ansæ on both sides of the planet. Denoting by A, B, C, and δ , the exterior bright ring, the interior bright ring, the dusky ring, and the diameter of the planet in the plane of the ring; also, for the distances measured in the direction of the ansæ from the nearest limb of the planet, let

<i>ab</i>	be	that to the interior boundary of C.
<i>ac</i>	"	exterior do.
<i>ad</i>	"	interior boundary of B.
<i>ae</i>	"	exterior do.
<i>af</i>	"	interior boundary of A.
<i>ag</i>	"	exterior do.

In 1851 the so-called dark ring appeared as if divided by a black stroke near C into two parts, of which the interior ought, according to analogy, to be regarded as a ring completely separated from B, whilst the exterior part proved in some measure only a prolongation of B with fainter light. But last year no trace of this division was to be discerned even when the images were at their best, clearly in consequence of changes which had taken place in the ring-system: the distance *ac* therefore could not be measured last year, there being no such point as *c* to fix upon; the distance *af* has been omitted because it was not measured in 1851, a circumstance which M. Struve was not aware of until he had finished the measures of 1882, having not referred to them before in order to avoid any possibility of bias. There are therefore (besides the diameter of

the planet) only four elements of comparison with the measures in 1851.

The results from the six series, after reducing to the mean distance of Saturn, 9'5389, and correcting for the phase, are (the measures of the two ansæ were kept quite distinct from each other):—

	<i>ab.</i>	<i>ad.</i>	<i>ae.</i>	<i>ag.</i>
For the preceding ansa	1''·44	3''·72	8''·27	11''·20
„ following ansa	1 ·53	3 ·61	8 ·13	11 ·20
Difference	+0 ·09	-0 ·11	-0 ·14	0 ·00

These measures show no very appreciable difference in the measures for the two ansæ, the only instance in which any real divergence might be suspected being in the united breadths of the dusky and inner bright rings, C and B, which would appear to be 6''·83 in breadth on the western side, and only 6''·60 on the eastern. Curiously enough, however, the measures made of this quantity by M. Meyer in 1880 and by Prof. Holden in 1879 and following years agree in making the *eastern* ansa the broader; Prof. Holden has remarked, however, that he was somewhat sceptical as to the reality of this difference, the more so that whilst the difference between the breadth of the eastern and western ansæ remained fairly accordant in different years, the measures of the actual breadths themselves showed a difference of very nearly 1'', one seventh of the total amount.

Considering then that his measures indicate no eccentricity of the position of the planet within the ring-system, M. Struve has combined the measures taken on both sides. His principal difficulty was in fixing on the point denoted by *d*, as it was always somewhat uncertain precisely where the bright interior ring B terminated towards the inside, and the dusky ring began, an actual separation of the two not existing, but only a more rapid shading of lighter into darker appearance. Nevertheless it was found by comparing the measures made on different evenings that the same point was, as a matter of fact, determined, and that the probable error of the mean amounted, even for this point, to only about 0''·045.

Now taking the arithmetical mean from the results obtained for the two ansæ, and comparing them with those obtained in 1851*, we find:—

	1851.	1882.	Difference, 1882—1851.
<i>ab</i>	1''·61	1''·49	-0''·12
<i>ad</i>	3 ·64	3 ·66	+0 ·02
<i>ae</i>	8 ·24	8 ·20	-0 ·04
<i>ag</i>	11 ·03	11 ·20	+0 ·17

Looking at these numbers, the small difference in the values found for *ab* gives at the most but very small indication of further

* The figures for 1851 differ slightly from those published in that year because the definitively determined value of the micrometer-screw is here introduced.

approximation of the rings towards the body of the planet, and none at all of any such movement in the mass. On the other hand, the difference, *in a different direction*, between the values of *ab* and *ag* suggests the supposition (which would be by no means in contradiction to previous results) that the breadth of the whole ring-system has sensibly increased.

In conclusion, M. Struve hopes that these measures will lead future observers to follow up the subject at greater intervals of time.

That changes *do* take place in the ring-system is sufficiently proved by the disappearance of the dark little space observed at *c* in 1851. Another indication of such changes is the fact that some observers have clearly perceived a division in the exterior bright ring A which others have contested. M. Struve himself has frequently had the impression of such a division at about $\frac{1}{3}$ of the breadth of the ring from its outer edge; but this was principally noted when the images were somewhat tremulous, the impression either wholly or partly passing away when they became steady.

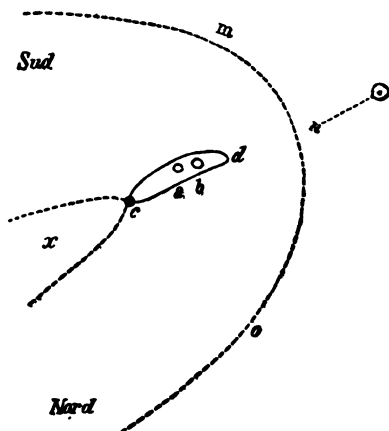
M. Schiaparelli a year and a half ago, in the splendid climate of Milan, thought he saw this division distinctly, but (as he wrote himself to M. Struve recently) failed to see it with certainty last autumn, and therefore now ascribes the appearance to periodical cloud-formation or something of that sort.

The distance *ag*, or that from the limb of the planet to the outer edge of the exterior ring, is measured with the greatest certainty. But considerably less accuracy is claimed for the measurement of the diameter of the planet, as errors made on the two sides tend to increase the error in the deduced diameter. But after repeating these measures very frequently with the view of cancelling casual errors, M. Struve has found for the mean δ last year (reduced to the mean distance of the planet) $17''.76$ as against $17''.56$ in the year 1851. Combining this with twice the value of *ag*, we obtain for the exterior diameter of the exterior ring $40''.16$, which is $0''.54$ greater than that determined in 1851. But this difference is considered by M. Struve to be chiefly due to the uncertainty in the measures of the diameter of the planet.

PROF. SCHMIDT ON THE NUCLEUS OF COMET II. 1882*.—Dr. Julius Schmidt has published some interesting remarks on the nucleus of this, the great comet of last year, in 'Astr. Nach.' No. 2499, accompanied by a drawing, which we here reproduce. The latter has a considerable resemblance to Mr. Prince's drawing in the 'Monthly Notices' for January (p. 85), to which Dr. Schmidt refers as a good representation of what was seen also by himself; but Mr. Prince's engraving gives the appearance of the comet, not as it would be seen in an inverting telescope, but as if in direct vision; and he has omitted to indicate the direction pointing

* Astr. Nach. No. 2499.

towards the Sun. This is laid down by the line $n \dots \odot$ in Schmidt's figure, where mno is the periphery of the coma, x the dark space behind the nucleus, which at first extended a distance of 14° up to the western end of the tail, and cd the nebulous or fan-shaped nucleus. In this Dr. Schmidt saw, on October 3rd, the true nucleus at the point c , the western end of the fan. But he saw it in that



place on that night only, and then (although the circumstances were very favourable) only after the closest examination with a powerful eyepiece. With eyepieces of smaller power, c did not seem to be the nucleus, on account of the greater general brightness of the whole surrounding nebulous mass; but the maximum of light in cd showed itself at the eastern end, condensed near bd , so that this part only would be referred to in the determination of place. If, therefore, c was the true place of the nucleus, the comet's place resulting from the observations would be uncertain by the distance cb (or cd). After the 3rd of October, the nucleus at c was never actually visible with certainty, and its approximate place had to be inferred. Dr. Schmidt could only see distinctly the two small bright disks marked a and b , from $1''$ to $3''$ in diameter, the distances of which in right ascension he was able to measure repeatedly, though only at times when the twilight was bright, about $17^h.3$ Athens mean time. In several respects the phenomena exhibited presented a remarkable similarity to those of Comet III. 1862 (the comet connected with the August meteors), Dr. Schmidt's careful and detailed description of which is well known. But in that case the part which must be considered to be the nucleus proper always continued to be also the brightest part; and there was a vibratory motion in the fan* which did not exist in the comet of 1882. Dr. Schmidt remarks finally on the prematureness of attempting any definitive determination

* "Sein schmaler Fächer zeigle enorme Veränderungen im Positionswinkel."

of the orbit of this comet, or of the changes it underwent in passing through part of the solar corona, until after a full discussion of the changes in the nucleus, so as to arrive at some certain conclusion with regard to the part which was actually taken in making the observations during the time that these changes were in progress. This can only be obtained by a careful comparison and analysis of all the observations of this remarkable comet, made by the various astronomers who observed it from the time of its memorable perihelion passage on the 17th of September. We shall probably have occasion to recur to this subject when all the materials for doing so shall have been published.

HEVELIUS.*—The name of Hevelius is known to all who have the slightest acquaintance with the history of astronomy, but a knowledge of his labours is confined to a much smaller number; and his principal work, and the one which perhaps best displays the enthusiasm of the man for his favourite science, and the zeal and energy with which he pursued its study, is known to fewer still. In view, therefore, of the rarity of this work, the '*Machina Celestis*' (all but a few copies having been destroyed by fire soon after its production), Mr. Prince has felt impelled to reprint three of the most interesting chapters with an English translation. The chapters chosen are the eighteenth, twenty-first, and twenty-second, describing Hevelius's Helioscope for solar observations, his great telescope, and a design for an observatory, and they give a vivid idea of the great difficulties with which the early astronomers had to contend.

The illustrations given by Hevelius in the chapters in question are likewise reproduced on a reduced scale, and, by way of contrast to his unwieldy telescope of 150 feet in length, representations of two of Mr. Prince's own instruments are added. Portraits of Hevelius and Gassendi are also given, the latter, in connection with some notes on the observations of the planets and particularly of Saturn, made by these two painstaking astronomers, showing how optically imperfect as well as cumbersome were the telescopes with which they had to work. The now celebrated observation by William Ball is also alluded to, and Mr. Prince, to whom we are so largely indebted for the correction having been made, shows, as every one now knows, that the idea of his having discovered the principal division in the ring is wholly due to a misconception. Altogether Mr. Prince has produced a very attractive and pleasing little book, and one calculated to increase our respect for the zeal and perseverance of those fathers of our science who did such worthy work under disadvantages so great.

* The Illustrated Account given by Hevelius in his '*Machina Celestis*' of the Method of Mounting His Telescopes and Erecting an Observatory. Reprinted from an Original Copy, with some Remarks. By C. Leeson Prince, 1882. For private circulation.

MR. PRINCE'S OBSERVATIONS OF THE GREAT COMET*.—In this second little work Mr. Prince has given a simple and straightforward account of his observations of the two great astronomical phenomena of last year, the Great Comet and the Transit of Venus. The account of the former is supplemented by a brief review of some of the principal opinions formerly held as to the nature of the tails of comets, and by a list of comets with one of which the Great Comet may possibly be identified. The work concludes with a reproduction of Baudouin's account of the supposed discovery of a Satellite of Venus by Montaign, the author seeming by no means so inclined to authoritatively decide against the real existence of the satellite as most astronomers would be at this time. The book, which is very neatly got up, contains seven illustrations. Of these the most important is a sketch of the head of the comet on Oct. 23, the same sketch, indeed, as that mentioned in Prof. Schmidt's paper referred to above.

A NEW MECHANICAL STAR-MAP †.—This is a map of all the stars seen in the latitude of Greenwich, but covered by a card from which a circular portion, bounded by the projection of the rational horizon of Greenwich, has been cut out. This circular opening therefore shows all the stars visible here at any one moment. The opening is covered by a circular piece of tracing-paper on which curves are drawn, from which the altitude or north-polar distance of any star may be inferred within half a degree or perhaps even less, and the azimuth perhaps to the nearest degree. A wooden knob underneath the star-map enables it to be turned with ease, and the size of the graduated circle round the map renders it possible to set it for use with a considerable amount of precision, the map thus giving a clear view of the stars above the horizon at any given time and of their apparent positions, and solving most of the problems usually referred to the celestial globe with about equal accuracy, and in some instances with greater quickness and convenience. A descriptive pamphlet accompanies it. It seems a handy and useful contrivance, and is carefully executed.

THE ASTRONOMICAL PRIZES OF THE ACADEMY OF SCIENCES ‡.—The French Academy of Sciences has awarded the Lalande Prize to M. Souillart, Professor to the Faculty of Sciences of Lille, for his investigations into the theory of the Satellites of Jupiter. The Damoiseau Prize for the revision of the theory of the Satellites of Jupiter was not granted; but an encouragement of two thousand francs was given to Dr. W. Schur for his recent determination of

* Observations upon the late Great Comet and Transit of Venus, made at Crowborough, Sussex, in the Year 1882, by C. L. Prince, F.R.A.S. With illustrations. Lewes: H. W. Wolff, 1883.

† The Aspect of the Starry Heavens, at any time of the Night all the Year round: Description of a new Mechanical Star-Map, and Examples illustrating its Use. By Theodore Grosse. Manchester: A. Ireland & Co., 1883.

‡ Comptes Rendus, Vol. xcvi. No. 14.

the mass of Jupiter and of the eccentricities of the orbits of the first and second satellites. Two Valz Prizes were awarded; the one to Dr. Huggins for his spectroscopic work, especially the determination of the motions of stars in the line of sight, and for the following photographic work—Photographs of stellar spectra, of the luminous spectrum of water-vapour, of the spectrum of Comet *b*, 1881 (the first ever photographed), of the spectrum of the great nebula in Orion, of the spectrum of Comet *a*, 1882, and of the solar corona with an uneclipsed Sun. The second Valz Prize was given to M. Cruls, director of the Rio Observatory.

THE TOTAL SOLAR ECLIPSE OF 1883, MAY 6.—The American expedition to Caroline Island to observe this eclipse is under the charge of Prof. E. S. Holden, director of the Washburn Observatory, Madison, Wisconsin, who intends to devote himself chiefly to the search for an intra-Mercurial planet. Profs. Hastings, of the Johns Hopkins University, and C. S. Pierce, of the American Coast Survey, are to undertake the spectroscopic observations; Mr. Rockwell, of Tarrytown, New York, and Lieut. Brown, of the American Navy, will observe the contacts and make eye observations, whilst the photographic work is to be undertaken by the two English observers, Messrs. Woods and Lawrance. Caroline Island, which has not been visited, so far as is known, since 1874, is a mere reef or collection of islets. Its population at that time consisted of about thirty natives and one Englishman.

The French expedition will be located on Sable Island, near Caroline Island. It is under the command of M. Janssen. The instruments to be employed comprise a short-focus telescope for spectroscopic work, a telescope which M. Trouvelot will use for drawings of the corona, and for the search for intra-Mercurial planets, and two equatorials, the one with five photographic cameras for intra-Mercurial planets, the other with three for photographing the corona.

THE GREAT COMET *b* 1882.—We learn from 'Nature' that Prof. Riccò observed this comet on April 6 with the 10-inch refractor of the Palermo Observatory as follows:—

M.T.	App. R.A.	App. Dec.
April at 8 ^h 21 ^m 29 ^s	5 ^h 58 ^m 5 ^s .93	−9° 4' 49".2.

The comet was a very faint nebulosity with an elongated nucleus containing two or more points. Prof. Riccò examined the Sun's limb for prominences on the day following the comet's perihelion passage, but found that they were neither unusually large nor numerous; and he concludes that, as no great effect was produced upon the motion of the comet by its course through the coronal atmosphere, so also no disturbance of the ordinary phenomena of the solar activity was occasioned by the same event.

ECLIPSE OF JUPITER'S FOURTH SATELLITE.—The following account by Dr. T. D. Simonton of an observation of the first eclipse of this satellite which has occurred since 1880, June 16, appears in the 'Sidereal Messenger':—

"As I had never noticed a predicted eclipse of the satellites to fail, by a minute even, it was a great surprise to see this 4th satellite, though faint beside two others near at hand, continue plainly visible minute after minute beyond the predicted time. And this continued up to 10^h 50^m (Washington time), when for the first time I was sure of a diminution of light. There was now a rapid waning, till at 10^h 55^m, if visible at all, the light of the body was excessively faint. This continued but a minute or two, however, when I was sure of it again by glimpses, and in a minute or two more, before 11^h 0^m, could see it steadily. As it now grew in brightness I ceased looking before 11^h 10^m. We have here, I suppose, a return of the conditions bringing about a stated eclipse of this satellite, though in this first predicted instance I doubt if for a single moment the whole body was brought within the sweep of the great shadow of its primary." The observation was made with a three-inch achromatic. The predicted times of disappearance and reappearance in the American 'Nautical Almanac' are 10^h 28^m 28^s.9, and 11^h 27^m 13^s.3 Washington Mean Time, corresponding to 15^h 36^m 41^s.0 and 16^h 35^m 25^s.4 G.M.T.

NEW NEBULÆ*.—M. E. Stephan, at the Observatory of Marseilles, has recently published a list of 96 nebulae which he has observed within the last two years. The great majority of these are new discoveries; but several seem to be observations of nebulae, the places of which in Herschel's Catalogue are slightly in error. Their mean places for the beginning of 1880 are as follows:—

No.	R.A.			N.P.D.			No. in Herschel's Catalogue.
	h	m	s	°	'	"	
42.....	11	9	8.45	71	14	8.7	2352
43.....	11	10	28.49	71	19	39.1	2356
44.....	11	10	36.52	71	17	35.0	2358
45.....	11	10	40.73	71	11	46.7	2359
58.....	14	4	57.55	69	49	12.8	3800 probably
85.....	17	50	44.21	71	38	33.5	4348
86.....	17	50	48.57	71	36	27.1	4349
95.....	23	37	51.90	64	35	26.2	5004

The two following are identified with two in Dreyer's Catalogue:—

No.	R.A.			N.P.D.			No. in Dreyer's Catalogue.
	h	m	s	°	'	"	
39.....	10	38	36.09	82	46	18.7	5534 probably.
90.....	18	59	30.57	96	10	23.4	5940, of which the polar distance is 2' wrong in the catalogue.

* Comptes Rendus, Vol. xvi. Nos. 9 and 10.

Ephemeris of D'Arrest's Comet.

THE following ephemeris, for the next period of the absence of moonlight, and which is calculated from M. Leveau's predicted elements, is extracted from 'Nature,' No. 702.

For Greenwich Midnight.

	a.			δ.			Log. Δ.	Log. r.
1883.	h	m	s	°	'	"		
April 23....	13	38	14	+ 11	13	7	0.2951	0.4649
25....	13	36	25	11	27	6		
27....	13	34	37	11	40	8	0.2927	0.4609
29....	13	32	50	11	53	2		
May 1....	13	31	3	12	4	7	0.2912	0.4569
3....	13	29	18	12	15	3		
5....	13	27	35	12	25	1	0.2906	0.4528
7....	13	25	55	12	33	9		
9....	13	24	18	+ 12	41	7	0.2908	0.4486

The discovery of D'Arrest's comet on April 4 at Strasburg Observatory was announced in the 'Dun-Echt Circular,' No. 76; but the announcement was corrected shortly after, the object which was mistaken for the comet proving to be a small nebula. The circular likewise gave the *daily motion* of the object. This was not from any error in observation on Dr. Hartwig's part, who simply announced the place of the supposed comet, but was inserted, *from the ephemeris*, by Prof. Krueger in his telegram to Harvard College, for the assistance of American astronomers, who were not, he believed, in possession of an ephemeris.

Ephemeris of Comet b, 1882.*

By CARL STECHERT.

For Berlin Midnight.

	a.			δ.			Log. Δ.	Log. r.
1883.	h	m	s	°	'	"		
April 29....	6	10	35.3	- 6	50	0	0.64874	0.60572
30....	6	11	12.4	6	45	37		
May 1....	6	11	49.9	6	41	20		
2....	6	12	27.7	6	37	10		
3....	6	13	5.7	6	33	6	0.65782	0.61082
4....	6	13	44.0	6	29	9		
5....	6	14	22.6	6	25	19		
6....	6	15	1.4	6	21	35		
7....	6	15	40.5	- 6	17	57	0.66652	0.61583

Taking the brightness as unity on February 8, when the comet, according to Schmidt, was invisible to the naked eye, the brightness on April 29 would be 0.16, and on May 7 0.14.

* Astr. Nachr. No. 2500.

Elements of Comet a, 1883.*

By MESSRS. CHANDLER and WENDELL.

THE elements are computed from observations made at Cambridge, Mass., on 1883, February 24, March 5, and March 17, and one made at Albany, New York, on March 5. Parallax and aberration have been corrected for.

Perihelion passage = 1883, February 18^g3572, G.M.T.

Long. (Per.—Node) .. 110° 52' 20''·7 }
 Long. Node 278 7 40 ·7 } Mean Equinox 1883·0.
 Inclination 78 4 40 ·2 }

Log. q 9·8807542C—O; $\Delta\lambda \cos \beta = +4''$; $\Delta\beta = +2''$.

Constants for 1883·0.

 $x=r[9\cdot3955398] \sin(145^\circ 31' 48''\cdot4+v)$ $y=r[9\cdot9902891] \sin(359 6 17 \cdot6+v)$ $z=r[9\cdot9960383] \sin(87 26 31 \cdot4+v)$

The elements and ephemeris given in the 'Dun-Echt Circular,' No. 73 ('Observatory,' No. 72, p. 131), are vitiated by an error in the observations made at Kiel on March 8.

Ephemeris of Comet a, 1883†.

By J. RITCHIE, Jun.

(Computed from the above elements.)

For Greenwich Midnight.

1883.	α .			δ .			Log. r .	Log. Δ .	I.
	h	m	s	°	'	"			
April 25....	4	56	45·8	11	6	58	0·15276	0·30830	0·098
27....	5	1	39·7	10	27	51			
29....	5	6	24·2	9	49	53	0·16946	0·32560	0·084
May 1....	5	11	0·0	9	12	57			
3....	5	15	27·9	8	37	1	0·18557	0·34202	0·072
5....	5	19	48·4	8	1	59			
7....	5	24	2·0	7	27	48	0·20109	0·35757	0·063
9....	5	28	9·3	6	54	24			
11....	5	32	10·6	6	21	45	0·21606	0·37228	0·055
13....	5	36	6·4	5	49	44			
15....	5	39	57·0	5	18	20	0·23049	0·38619	0·046
17....	5	43	42·8	4	47	30			
19....	5	47	24·0	4	17	8	0·24445	0·39935	0·043
21....	5	51	0·9	3	47	15			
23....	5	54	33·4	3	17	50	0·25787	0·41177	0·038

The brightness at time of discovery is taken as unity.

* Dun-Echt Circular, Nos. 75 and 77, and Astr. Nachr. No. 2504.

† Dun-Echt Circular, No. 77.

Astronomical Memoranda, 1883, May.

Sun. May 1, sets $7^h 21^m$, rises $16^h 33^m$; May 11, sets $7^h 36^m$, rises $16^h 15^m$; May 21, sets $7^h 50^m$, rises $16^h 2^m$; May 31, sets $8^h 3^m$, rises $15^h 51^m$.

Equation of Time:—Sun before Clock, May 1, $3^m 0^s$; May 11, $3^m 48^s$; May 21, $3^m 39^s$; May 31, $2^m 38^s$.

Sidereal Time at Mean Noon:—May 1, $2^h 36^m.3$; May 11, $3^h 15^m.7$; May 21, $3^h 55^m.1$; May 31, $4^h 34^m.6$.

<i>Moon. rises.</i>			<i>sets.</i>			<i>sets.</i>			
	h	m		h	m		h	m	
May 1	1	14	9	May 11	11	57	May 21	16	16
	2	14	36		12	12	22	..	<i>rises.</i>
	3	15	3		13	12	23	..	9 14
	4	15	33		14	13	24	..	10 0
	5	16	6		15	13	25	..	10 41
	6	..	<i>sets.</i>		16	13	26	..	11 16
	7	..	8 27		17	14	27	..	11 46
	8	..	9 35		18	14	28	..	12 13
	9	..	10 34		19	15	29	..	12 39
	10	..	11 21		20	15	30	..	13 4
							31	..	13 32

New Moon, May 6, $9^h 58^m$; First Quarter, May 13, $10^h 54^m$; Full Moon, May 21, $15^h 12^m$; Last Quarter, May 29, $2^h 23^m$.

There will be an eclipse of the Sun on May 6, wholly invisible at Greenwich, and visible as a total eclipse only over the South Pacific Ocean, the only land from which it will be seen being Flint and Caroline Islands.

Mercury is an evening star, at greatest elongation, $21^\circ 48'$ E., on May 14, 2^h , when he is favourably placed for observation. He is in conjunction with Saturn, May 1, 16^h .

Venus is a morning star. It is in conjunction with Mars, May 9, 18^h , Mars being $0^\circ 48'$ N. of Venus. Diameter:—May 1, $13''.5$; May 31, $11''.7$. Illuminated portion of disk, 0.822 on May 15.

May 1, R.A. $0^h 18^m.1$, Dec. $0^\circ 12' N.$, tr. $21^h 42^m$, rises $15^h 35^m$
31, 2 32 .6, 13 11 N., 21 59 14 45

Jupiter is an evening star, in conjunction with μ Geminorum May 22, 20^h , the star being $0^\circ 50'$ S. of the planet. Diameter:—May 1, $32''.0$; May 31, $30''.3$.

May 1, R.A. $5^h 57^m.1$, Dec. $23^\circ 25' N.$, tr. $3^h 20^m$, sets $11^h 37^m$
31, 6 23 .5, 23 22 N., 1 49 10 4

Saturn is in conjunction with the Sun, May 20, 11^h .

Uranus is stationary, May 27, 8^h .

May 1, R.A. $11^h 22^m.7$, Dec. $4^\circ 53' N.$, tr. $8^h 45^m$
31, 11 21 .6, 4 59 N., 6 46

Neptune is in conjunction with the Sun, May 9, 1^h .

Phenomena.

		G. M. T.				G. M. T.	
		h	m			h	m
May	2	8	15	J. ii. Ec. R.		May	17
		11	21	J. iii. Tr. I.			10
	6	8	12	J. iii. Ec. R.			48
		10	29	J. i. Oc. D.			χ Virginis
	7	7	39	J. i. Tr. I.			Oc. R. 238°.
		8	34	J. i. Sh. I.			10
		9	56	J. i. Tr. E.			56
	8	8	8	J. i. Ec. R.			B.A.C. 4259.
		9	10	50	J. ii. Ec. R.		Oc. R. 242°.
	13	8	52	J. iii. Oc. R.			18
		9	18	J. iii. Ec. D.			8
	14	9	40	J. i. Tr. I.			32
		15	10	3	J. i. Ec. R.		J. ii. Sh. E.
	16	8	42	J. iv. Sh. E.			10
		9	7	J. ii. Oc. D.			18
	17	9	27	χ Virginis			J. iii. Oc. D.
				Oc. D. 73°.			22
		9	34	B.A.C. 4259			9
				Oc. D. 68°.			1
							J. i. Oc. D.
							15
							15
							B.A.C. 5663
							Oc. D. 160°.
							15
							43
							B.A.C. 5663
							Oc. R. 240°.
							23
							8
							28
							J. i. Tr. E.
							9
							10
							J. i. Sh. E.
							9
							13
							J. iv. Oc. D.
							24
							9
							13
							J. iv. Oc. D.
							25
							8
							21
							J. ii. Sh. I.
							9
							50
							J. ii. Tr. E.
							30
							8
							12
							J. i. Tr. I.
							8
							47
							J. i. Sh. I.
							31
							8
							22
							J. i. Ec. R.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

ERRATA IN No. 72.

Page 118, line 39, for night of 1874, Feb. 4, read night of 1872, Feb. 4.
131, line 23, for Long. Per. read Long. Node.

Publications received:—E. C. Pickering, *Observations of the Transit of Venus, December 5 and 6, 1882, made at the Harvard College Observatory—Thirty-seventh Annual Report of the Director of the Astronomical Observatory of Harvard College—First Circular of Instructions for Observers of Variable Stars, acting in co-operation with the Harvard College Observatory* (University Press, Cambridge, Mass.).—D. P. Todd, M.A., *An Account of the Observations of the Transit of Venus, 1882, made at the Lick Observatory, Mount Hamilton* (Amer. Journ. of Science, Vol. xxv.).—Theodore Grosse, *The Aspect of the Starry Heavens at any Time of the Night all the Year round: Description of a New Mechanical Star-Map* (Ireland & Co., Manchester).—P. F. Denza, *Osservazioni del passaggio di Venere sul disco solare, fatte all' Osservatorio del R. Collegio Carlo Alberto in Moncalieri* (Rendiconti del R. Istituto Lombardo, Serie ii. Vol. xvi.).—*Osservazioni delle Meteore Luminose negli anni 1882 e 1883, Anni xiii. e xiv.*

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 74.

JUNE 1.

1883.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1883, May 11.

E. J. STONE, M.A., F.R.S., *President*, in the Chair.

Secretaries: J. W. L. GLAISHER, M.A., F.R.S., and
E. B. KNOBEL.

THE Minutes of the last Meeting were read and confirmed.

Mr. Knobel. Sixty-eight presents have been received since the last Meeting of the Society; among others a most beautiful photograph, upon an opal plate, of the great Nebula of Orion, presented by Mr. Common, which almost eclipses the photograph we have already on the walls.

Captain Noble asked if the 'Monthly Notices' could not be brought out a little earlier. At present, as he had to leave home very early in the morning, he did not get his copy until the day after the Meeting. He thought some arrangement might be made by which the 'Notices' might be delivered on the day before the Meeting.

Mr. Knobel pointed out that the corrections which had to be made by the authors of the papers occasioned great delay; but, so far as the officers of the Society were concerned, there was no delay which could possibly be avoided.

Captain Noble thought that if gentlemen did not correct their papers in time they should go in uncorrected. He quite believed what *Mr. Knobel* had stated, that the blame was not referable to the officers of the Society; for he recollected that when *Mr. Carrington* edited the 'Notices' there was delay, and again when *Mr. Cayley* edited them: it had been the same with all the editors. The fault therefore rested with somebody outside, and he was disposed to blame the printers; and he thought they should be made to

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understand that it was a part of their contract to deliver the copies within a specified time.

Mr. Glaisher thought the publication was better as it was. The 'Notices' were always delivered before the following Meeting. He would much rather that the publication were delayed than that the papers should go uncorrected and inaccurate. In former times, in order to save a few days, the 'Notices' had been published in a state which was a discredit to the Society, because they were hurriedly printed. It was a rule that was adhered to now, that the 'Notices' should be published "before the next Meeting." That was done, but at what cost nobody but those who are concerned in their production could know. He would rather have delay, and secure that the publication should be a credit to the Society. He did not agree with Captain Noble that the printers were to blame. They were under contract to issue the 'Notices' "before the next Meeting;" they got it out in time, and they could not do it before. He should decidedly oppose any publication at an earlier date; very few societies published "before the next Meeting."

Prof. Pritchard said that this was a very old tale. He could not help thinking that Captain Noble was perfectly right from his point of view, and that the Officers of the Society were "righter" from their point. (Laughter.) The main point was that we should know what to expect. If we were to have the 'Notices' the day before the Meeting, very well; but if that was impossible let us submit.

The President. My own views are entirely on the side of our Secretaries. The first thing is to secure that the 'Monthly Notices' are properly printed, and then the earlier we can have them the better. But the first thing is that they should be accurate. I am quite sure it is not desirable that the 'Notices,' containing a great many of our best papers, should be printed until revised. When the proofs are forwarded for correction the authors are sometimes absent, and there may be the loss of several days; and if the Secretaries get out the 'Notices' as quickly as possible, I think that is as much as we can expect of them. I do not think we can lay down any hard-and-fast rule that the 'Notices' shall be delivered one or two days before the following Meeting. It would lead to difficulty in passing valuable papers for the press. The Secretaries do all they can to get the 'Notices' out punctually, and I think we can safely leave it in their hands.

Mr. Downing read a paper on the orbit of the binary star γ Coronæ Australis. Some years ago Prof. Schiaparelli published elements of the orbit of this star, and corrections to these elements have been determined by what is known as Klinkerfues' method, adopting the modifications proposed by Dr. Doberck. The elements thus corrected represent very accurately the whole series of observations published up to the present time. The corrections to Schiaparelli's elements are, however, small; and the chief object in bringing the subject forward has been to draw attention to the

circumstance that the periastron passage occurred in the early part of the present year, in the hope that observers in the southern hemisphere may be induced to make observations of this interesting binary before the present critical state of things has passed away.

Dr. Doberck considered that the method *Mr. Downing* had employed was the very best for correcting the elements of double-star orbits, after that these had been determined by aid of *Sir J. Herschel's* graphical method, and that it was on the whole superior to the method of determining the corrections from equations of condition of a form similar to those used in the correction of planetary and cometic orbits. It had not been much used, at least not in this country, and he hoped that *Mr. Downing* would make good use of it in the future.

Prof. Pritchard gave an account of his recent expedition to Cairo. He said:—

The work on which during the last two years I have been engaged, viz. the measurement of the magnitude of the stars visible to the naked eye from the pole to the equator, including at present all those brighter than the fifth magnitude, is now completed, and is virtually in the hands of the Society. From the point of view that it has been carried on in one uniform, practicable, and philosophical system of photometry, I think I may claim for it that it is the first attempt of the kind made in this country. Some of the general methods and results I have already from time to time communicated to the Society; and I should not now propose to occupy your time were it not that some facts, connected with my recent visit to Egypt for the purpose of completing the work, have come to light, which have an intimate bearing on the apparent “magnitudes” of the stars *as they are affected by our atmosphere*.

One necessary but somewhat difficult part of the work at Oxford consisted in ascertaining the law and the amount of the variation of the “magnitude” of a star as it gradually rises to its meridian height. More than this, it was essential that the apparent magnitude which it would attain, if viewed in the zenith, could be determined. Further still, it would be an object of interest if we could state what would be a star's brightness if our atmosphere were wholly removed, or, at all events, if that atmosphere extinguished no light.

It was not long before I found that the atmospheric effects on the intensity of a star's light at Oxford varied from night to night, and sometimes even during the same night, and this quite independently of the height of the star. Even on fairly fine nights the atmospheric effects on a star when within 25° of the horizon were very conflicting. *Laplace*, in the ‘*Mécanique Céleste*,’ has given an elaborate mathematical investigation of the effect of the atmosphere in absorbing the light of a star at various zenith-distances. The general expression which he derives is, however, unmanageable for numerical expression in its rigorous form. He accordingly makes certain assumptions which at once reduce it to great sim-

plicity, and, in fact, indicate that the alteration of a star's light, as measured in magnitude, varies with the secant of a star's zenith-distance excepting very near the horizon. This result means, in fact, that the atmospheric absorption of light varies with the thickness of the atmosphere through which the star passes, just as in the case with the glass wedge forming the photometer used in these researches. On submitting a large number of observations of stars whose light was measured at varying altitudes, I found that this law of the secant of the zenith-distance was fairly well satisfied at Oxford down to zenith-distances not exceeding about 65° , but that for stars at lower altitudes than this the alterations in apparent magnitude were conflicting and not satisfactory. This might possibly be owing to local circumstances, and especially to that confluence of streams for which Oxford is remarkable. The apparent deviation from all law for such low altitudes did not, however, affect the results of the Oxford observations, because my aim and attention were for the most part directed to stars very near to or north of the equator. Nevertheless, knowing well the atmospheric discrepancies which sometimes occurred in the locality of my observations, and being desirous of making myself accurately acquainted with the effect of atmospheric extinction of light under better circumstances, I determined, if possible, to repeat the Oxford observations in some locality where the atmosphere is uniform and stable, if such could be found, in order to render my research complete. On inquiry I learnt that the climate of Upper Egypt, or of Cairo, would probably secure all the advantages which I sought.

Accordingly, and notwithstanding the great distance and the inconveniences of winter travelling, I determined to proceed to Cairo, taking with me one set of the instruments with which the Oxford observations had been made, and one of the assistants who had been greatly engaged in the work. Before moving thither I made a careful selection of the stars necessary to be observed in a specified order. A duplicate list was made of the same objects for observation, and these were to be carried on, in my absence, at Oxford by the senior assistant, who had had charge of the duplicate set of instruments. On arriving at Cairo, about the end of January last, I received the kindest and most valuable assistance from all the official personages, from Lord Dufferin and Sir E. Mallet downwards. Through their intervention the instruments were passed unpacked and unexamined at the various Custom Houses; and my assistant Mr. Jenkins was soon installed in the Khedive's admirable Observatory at Abbaseeyeh, on the borders of the desert, about three miles from Cairo. A more favourable locality it would be difficult to find. Not a day was lost in the commencement of the prescribed observations; and it may be interesting to remark that the first view I had of the more than ancient Pyramids of Gizeh was while standing by the side of the telescope with its adjuncts, which embodied all that was most modern in astronomical equipments. The Cairo observations proceeded without impedi-

ment or interruption. All the measurements prescribed were made in their due order; and out of the thirty nights during which Mr. Jenkins was lodged at Abbaseeyeh, twenty-six were fine, and admitted of measurements with a facility and continuity generally unknown at our European localities. The meteorology of Cairo in February is before you. This much, however, perhaps it may not be, even here, out of place to remark, that, summing up one's impressions of Egypt in a few words, I should say that Egypt is a land of a succession of surprises and of the consilience of the heterogeneous. Man, moreover, is there seen in the civilization of the time of Abraham, but with the marks of the oppression of 4000 years upon him. The climate is so exquisite that mere existence is a conscious enjoyment, but where some three millions or more of human beings are degraded enough to be content to feed, lodge, and clothe at perhaps $2\frac{1}{2}d.$ per day or less. I pass over incidents which daily excited our utmost interest in this amazing land, our present subject being rather the stars of the heavens than the wonders of Egypt; for wonders there abound. The necessary and prescribed observations having been made, Mr. Jenkins returned, by sea direct, with the instruments to London. My own journey was less direct, being desirous of escaping as much as possible the wintery disagreeables of a voyage across the Mediterranean. With this view I divided the return voyage to Europe into two by a passage to Athens, a locality possessing to a scholar scarcely less interest than the Pyramids of Memphis or the Temples at Carnac. At Athens I had the satisfaction of a long interview with that eminent veteran in astronomical research, Dr. Schmidt. To me it seemed, and it still seems, a thousand pities that the invaluable results of many years of his labours still lie unpublished. On returning to Oxford towards the end of March, I immediately set to work on the reduction and discussion of the observations made at Cairo and Oxford; and have thereby now completed the work which nearly two years ago I set myself to perform. Some of the more interesting results of these researches are recorded on the diagrams and numerical expressions now presented to your notice. First, there are the curves representing the atmospheric absorption of light at Cairo due to varying zenith-distance. I have selected two out of seventeen of them, as deduced from stars observed during two nights. You will remark how nearly parallel these curves are, indicating the consistency of the observations and the uniformity of the nights. Underneath them, in red colour, I have drawn the curve of secants which theory would indicate, viz. that the absorption, measured in magnitude, varies as the length of the atmospheric path traced by the light of the stars, excepting very near the horizon.

Underneath these I have drawn similar curves for the variation of the atmospheric absorption at Oxford due to variations in zenithal distance. You will see how inferior they are to the Cairo results, how they fairly follow the theoretical law as far as a

zenith-distance of 60° , and then fall away into comparative disorder. On a very few fine nights, however, even at Oxford this disorder at low altitudes disappears to a great extent. The juxtaposition of the Oxford and Cairo curves will explain the reason for the Cairo expedition. The results of both sets of observations are embodied in the formulæ:—

Atmospheric absorption

at Cairo = $\cdot 187 \times \text{sec. Z.D. in "magnitude."}$

at Oxford = $\cdot 253 \times \text{sec. Z.D. in "magnitude."}$

Diff. = $\cdot 066$ of one "magnitude."

From this it follows that the whole effect of the atmosphere at Cairo is to diminish the brightness of stars seen in the zenith by about $\frac{2}{10}$ of a magnitude only, and at Oxford by about $\frac{1}{4}$ of a magnitude. The difference is very slight, in fact quite undiscernable by eye estimation. If, however, we look at the heavens at about the altitude most natural or convenient, viz. about 30° high, then at Cairo the stars will be brighter than in England by about $\frac{1}{3}$ of a magnitude, and consequently many more faint stars should become just visible at Cairo than can be seen at Oxford. This was in fact the case. I will conclude this part of the subject by exhibiting to you a matter of perhaps some little interest in itself, and which shows how consistent and reliable the observations are. I have deduced from the absorption formulæ computed for Oxford what would be the magnitude of Sirius if viewed in the absence of our atmosphere: and then I have also deduced this magnitude of Sirius from independent observations and computations made at Cairo.

The two results are:—

Unabsorbed magnitude of Sirius

from Observations at Cairo = $-1\cdot 21$

from Observations at Oxford = $-1\cdot 14$

Diff. = $\cdot 07$

a difference of only $\frac{1}{13}$ of a magnitude.

Finally,

Sirius in the zenith at Oxford = $-0\cdot 96$ magnitude.

Sirius in the zenith at Cairo = $-0\cdot 95$ magnitude.

i. e. the real apparent magnitude of Sirius is just *two* magnitudes brighter than a star of the reputedly first magnitude, on the scale that Polaris shall be regarded as $2\cdot 05$ magnitude, a scale which I believe more nearly represents Argelander's estimation of magnitudes than perhaps any other.

The Meteorology of Cairo in February last is as follows:—

Meteorological Observations, February 1883.

	Cairo.	Oxford.
Maximum shade temperature for month..	69 ^o ·2	54 ^o ·4
Minimum do. do. ..	42 ^o ·9	30 ^o ·7
Maximum solar radiation.....	153 ^o ·2	98 ^o ·8
Duration of sunshine	192 ^h	85 ^h
Number of Wedge extinctions made in February	3385	997

Mr. Knobel. I would ask whether the book referred to by Professor Pritchard is that by Bouguer?

Prof. Pritchard. That is the book I refer to.

Mr. Knobel. There is no copy in the British Museum, but there is a copy in the Cambridge University Library, and I believe there is a copy for sale at present in Berlin.

Prof. Pritchard. I am glad to hear the Bodleian Library is not at fault. It was a matter of some interest to understand the precise method that Bouguer pursued.

Capt. Noble. I think Professor Pritchard's observations are of peculiar value in connexion with the question discussed on the other side of the Atlantic as to the erection of observatories at great heights. Prof. Pritchard's paper shows that even at Oxford only a quarter of a magnitude is lost, and at Cairo 0·187 of a magnitude, so that the difference is not very great; and the question arises as to the use of sticking observatories upon very high mountains in order to save so small a proportion of light. (Hear, hear.)

The Astronomer Royal. I should like to ask a question with reference to the law of sec. Z.D. for the atmospheric absorption. As far as I understand the question, this is merely an approximate law: it cannot be rigorously true down to the horizon (Professor Pritchard, No!), because if it were, a star would be invisible at the horizon. I would therefore ask how far it is sensibly true, because the absorption will really increase in a different way from that assumed by this law. And connected with this is the question of what scale of magnitude was adopted by Professor Pritchard.

Prof. Pritchard. What the Astronomer Royal has said is exceedingly to the purpose, and I think if it had not been said, it would have been a pity. Laplace himself stops his theory at about 80°. At Cairo I could extend my observations to 80°, but at Oxford I cannot go beyond 60°; and I suspect this fact means that the lower strata of our Oxford atmosphere are so variable, and so dependent on local circumstances, that this theory of absorption of light as well as the theory of the refraction of light (pardon me for daring to mention that term) does not at very low altitudes rigorously represent facts. Knowing all this, in my final results I have been most careful never to make anything depend on stars

much lower than the equator. The law will hold at Cairo to 80° . Laplace breaks off and makes a remark, which, had he known more of the subjects in a practical sense, I think he would scarcely have made. I do not believe that much beyond 80° anywhere in the world this law of the secants will hold. It might probably hold if the atmosphere were constant near the surface of the Earth; but the meteorological circumstances vary so tremendously according to the geography of the place and from its surroundings, that no dependence at all can be placed on the law of extinction. It must not be forgotten that the amount of accuracy aimed at in my researches is about the $\frac{1}{8}$ of a magnitude, on the average. I believe I have in general attained to it, and I do not think that hitherto in England any such general precision as the tenth of a magnitude has been consistently reached and maintained.

The Astronomer Royal. I think Professor Pritchard misunderstood me: my point is, that the law will not hold at large zenith-distances. It is not a question of the degree of accuracy of the law at 60° or 70° Z.D., but that near the horizon the law would fail, and would fail by large quantities when you get very near the horizon, even in a perfect atmosphere.

Prof. Pritchard. I don't know.

The Astronomer Royal. It must be so, because, if you take the secant it becomes infinite at the horizon.

Prof. Pritchard. No doubt about that. It holds at least as far as 80° from the zenith at Cairo; and all I say is, "Very well, don't observe stars beyond that zenith-distance." As to the scale of the "*light ratio*," the scale I have adopted throughout is Pogson's scale, viz. 2.512, a number whose logarithm is .4. As Captain Noble says, the investigation before us bears most intimately upon the interesting and important question he referred to; but I cannot now enter into it. It is, as he knows, a vexed question; still I do think this investigation goes a considerable way to settle it; practically, I think it has been pretty well settled before. Probably the conclusion suggested is this: stop where you are, and do your best.

Thanks were voted to Professor Pritchard for his paper.

Dr. Schuster. I should like to say a few words about some photographic results obtained during the last total solar eclipse in Egypt. I must express my regret that Captain Abney is not present, because whatever photographic results we have obtained are due to the excellent plates which he sent out. We had three photographic instruments at work. The object of one was to take photographs of the corona itself. The second was a photographic camera with a prism placed in front of it, that is to say, a spectroscope without a collimator. The third was a complete spectroscope. We obtained photographs in all the three instruments.

The direct photographs of the corona are of importance, because they show the variations of the corona from eclipse to eclipse. The question whether any regular change can be observed from

eclipse to eclipse—a change which might or might not coincide with the changes in sun-spots—is a question of very considerable importance to solar physics. If you look over the photographs taken during eclipses in the past 20 years, and compare them with each other, you will find no doubt that the corona does vary in a regular way with the state of the Sun's surface, although there are irregular minor changes.

The corona at the sun-spot *minimum* is much more regular than at the *maximum*. At the minimum we have a large extension in one direction, which I may call the equatorial extension, being somewhere near the solar equator. This extension has been seen during the last American eclipse to a considerable distance. Then we have near the solar poles a series of curved rays, which may be compared to the rays of the *aurora borealis*. At the *maximum*, practically there is no regularity at all. There are long streamers going up, sometimes in one direction and sometimes in another. This last year, near the sun-spot maximum there was absolutely no symmetry in the appearance of the corona.

What seemed to be most striking during the last eclipse was, first of all, the transparency of the streamers. We can trace sometimes one streamer through another, showing that the matter, whatever it is, must be very thin; and the next point of interest is, that the rifts start from the solar surface in a wholly irregular way. There is very often even a tendency towards the tangential direction at the lower parts of the rifts. The extent of a photograph depends very much on the sensitiveness of the plates. Our photographs extend a good deal more than a diameter, something like a diameter and a half, from the Sun's limb. One of the curious points during this last eclipse was that a comet appeared within, or a little more than, a solar diameter away from the Sun's limb. As it appears clearly in the photographs, it must have been very bright. It is a matter of some interest, though not of importance, to see whether any change in the position of the comet can be traced in the successive photographs during the eclipse. In the three quarters of a minute interval between the first and the last photograph it is calculated that there might have been an appreciable motion of the comet, and measurements seem to indicate such a small shift.

I now turn to the photographs taken with the camera and prism in front. That instrument, as you know, will give you an image of the prominences as often repeated as there are rays in the prominence. The plates we employed were plates sensible to the infra-red as well as violet rays. One of the prominences gave a great number of lines in the ultra-violet. It was, however, weaker in the red than one of the other prominences. The brightest lines in the prominences are not hydrogen. That fact was brought out in the last eclipse. The brightest lines were due to calcium. Besides these and the hydrogen lines, we have in the yellow the line D_3 , and the C line of hydrogen in the red, and we have a photograph of two prominence-lines in the ultra-red. In addition

to the prominences, we saw in the photographs certain short rings round the Moon, which means that at these places the light sent out by the gaseous part surrounding the Moon is not confined to the prominences. It is, as would be expected, the green coronal line which chiefly corresponds to one of those rings. This bears on the question whether the green coronal line appears stronger or weaker in the prominences than in the corona, or whether this green line K 1474 is seen all round the Moon. We can only trace the line very faintly in one of the prominences; so that K 1474 is a true coronal line, but hardly appears in the prominences. In addition to this ring corresponding to K 1474, we also see one corresponding to D₅. This is distributed more evenly all round the Moon, while K 1474 is stronger, but confined to a certain part of the Moon's limb. I come now to the results which have been obtained with the complete spectroscope. The slit, of course, crossed the Moon in the middle, and the spectra of the prominences and corona appear on both sides above and below the dark space corresponding to the position of the Moon. We are struck by the fact that some of the lines cross the Moon's disk, and especially the two lines H and K. This proves that the calcium lines H and K were so strong in the prominences that the light was scattered in our atmosphere, and reflected right in front of the Moon.

The prominence-lines are very numerous. We have in our photograph thirty lines belonging to them. We see the hydrogen lines, including the lines in the ultra-violet photographed by Dr. Huggins. Then, in addition to these, we have H and K and other calcium lines. The other lines are chiefly unknown lines; at any rate, we cannot fix them accurately.

Close to the Sun's limb we can only trace a continuous spectrum, a very strong one, going up to about a quarter of a solar diameter. Our photographs bear out the distinction, to which our President has drawn attention in the 1874 eclipse, between the inner corona and the outer corona, the inner corona being much stronger in light than the outer. The boundary at which this continuous spectrum ends corresponds to the extension of the inner corona. The continuous spectrum is stronger on the side where the prominences are weaker. In the corona we first of all see a very faint continuous spectrum, and in that continuous spectrum you can trace at G the reversal of the dark Fraunhofer lines. In addition, a series of faint true coronal lines can be traced in the outer regions of the corona. We have measured something like thirty of them; and there are a great many others we have not measured because they are too faint. These coronal lines exist chiefly on one side where the prominences are weakest; but they exist faintly also on the other. We have suspected at times that different lines appear on the two sides of the Moon; but we could not be certain of that. The coronal lines are noticed from G, or from a little to the red of G, up to H; they get fainter beyond; but there are a good number in the ultra-violet. The fact that these lines appear only in the outer

corona is strange ; but, looking carefully at the photograph of the inner corona, we notice that it is an over-exposed continuous spectrum, not bounded by straight lines, but by curves corresponding to the places where these lines appear, just as if the lines were really continued through the inner corona, but hidden by a superimposed continuous spectrum. Capt. Abney thinks he can get some of these lines in the inner part by dissolving away part of the film of the photograph ; but that is rather a dangerous proceeding when you have only one photograph. We have not traced any known substances in the solar corona. The greater number of the prominence-lines in the ultra-violet also are unknown, but they seem to be present in Dr. Huggins's photograph of *α Aquilæ*.

Mr. Ranyard. Have you made any drawings yet ?

Dr. Schuster. No ; but I suppose they will be made.

Mr. Ranyard. In regard to the comet, I do not think we ought to rely too much on any proof of its motion derived from the photographs during the eclipse, there are so many things to be taken into account in comparing them. There is not only the motion of the Moon but also a difficulty, due to irradiation, in determining the position of the centre of the Sun. I measured all the eclipse photographs that came into my hands, and found that the dark Moon was always elliptical, with the longest axis at right angles to the direction of its motion, and more elliptical than it should be. The reason of this is, because the lower strata of the Sun's atmosphere on the side that was left uncovered photograph themselves, and afterwards the strata on the opposite side would do the same ; and they appear to eat into the edge of the Moon, just as prominences do, but not to the same extent. This irradiation will shift with the Moon, so that it is impossible to determine the amount of irradiation on each side, and therefore to ascertain exactly where the Moon's centre is ; and of course the motion to be dealt with, when the whole eclipse lasted only 1 minute and 10 seconds, is very small indeed. With regard to the relative intensity of the line 1474, at various altitudes, what Dr. Schuster has said quite bears out former observations. There seems to be very fair proof that the 1474 line is the brightest line in the outer corona, whereas lower down in the prominences it is one of the fainter lines, ranking only fifth or sixth. But this is only one case amongst many which might be adduced to show that there is a change in the relative intensity of lines at different altitudes ; and in studying any part of the Sun on an ordinary occasion we have had to take that into account, for we then have the effects of different intensities corresponding to different heights mingled together. We are looking at the prominences through the corona, and the corona too is seen through light dispersed by our atmosphere from the prominences and the lower brighter regions of the Sun. My own photographs do not bear out the statement that there is an outer and an inner corona, and no photograph that I have seen shows anything like a stratification, or a sudden

decrease of brightness. Of course there are sudden photographic changes very low down, but at any such distance as half a solar radius there is no sudden decrease of brightness of the corona in any of the photographs I have seen, and certainly not in my photograph.

The Astronomer Royal. There is one question I would put to Dr. Schuster as to the thickness of the rings corresponding to 1474 K and D₃ in the photograph of the corona. I did not gather whether he could form any estimate of the height to which they extend.

The President. I should like to know whether in any photograph of the outer corona you have traced any distinct diminution of the brightness of 1474 line, whether it gradually faded away. That was the case with the eye observations made in the eclipse of 1874; we found the line got fainter as we receded from the centre, and we ultimately lost it from the extreme faintness to which it faded. With regard to the sharp distinction between the outer and inner corona, it was strongly marked in the eclipse of 1874. It was so strongly marked on that occasion that it required something of an effort to pass from the inner corona to the outer corona; and I saw people who were sitting down side by side making drawings—some of them drawing the inner corona and some drawing the outer corona. It was only when it was pointed out to him that one of them saw the outer corona. That shows, I think, that there was a well-marked difference between them.

Mr. Ranyard. As to sudden alterations in brightness, we know that observers in drawing nebulae will give them different dimensions.

The President. The outline of the inner corona appeared to me to be perfectly distinct, and it appeared to require an effort to pass from that to the outer corona.

Mr. Ranyard. This was an eclipse in which the Moon's diameter was nearly the same as the diameter of the Sun, and there was consequently an enormous illumination of the atmosphere, so that we must not be too positive in saying any particular light comes from this or that part of the corona.

Dr. Schuster. I do not quite follow Mr. Ranyard's reasoning about the comet; but this is not a question of any importance, and I only mention it incidentally. With regard to the inner corona, I did not mean to imply by what I said that there was a sharp line or boundary between the inner and the outer corona, but in looking at the photographs you can draw a very fair distinction between the places where the corona was over-exposed and between those that were not. There is a distinct inner corona. It is distinct and over-exposed, much over-exposed at certain points. The outline of the over-exposed region is not spherical, but extends more towards some parts than it does towards others; and it also confirms the photographs taken by Dr. Huggins, since his inner corona is of the same

shape as that photographed in Egypt. If you look at the photograph of the spectrum of the corona, there is no doubt about the distinction between the outer and inner part. You see at one place simply a strong over-exposed continuous spectrum, and at a certain point there is a definite boundary, and the continuous spectrum becomes much weaker. Then, as regards the 1474 line, I do not know whether I made myself perfectly clear that we get the line in the shape of a ring, simply by using the camera with a prism in front of it, but without a collimator.

The Astronomer Royal. What is the thickness of the ring? Can you form an estimate?

Dr. Schuster. No, it is difficult to say. It is a sharp ring on the inner side, then gradually fades away; then there is an enormous amount of light and a continuous spectrum coming in over all the remainder of the corona, so that, as it simply fades away gradually, we cannot form any idea of its actual height.

The Astronomer Royal. There is no distinct boundary outwards?

Dr. Schuster. No; there is no distinct boundary outwards; it fades away until there is nothing left. With regard to the remark that Mr. Ranyard made, so far as the prominence-lines are concerned, if you get them over the corona they may come from the prominences; but if a line is strongest in the corona, it cannot be due to a prominence. The light scattered about by the atmosphere must be faint compared with the original light; and if you can only just see some lines in the corona itself, and not at all in the prominences, we cannot, by any use of the imagination, say they are reflected lines from the prominences; they must be coronal lines due to the part of the corona where we see them.

Mr. Ranyard read a paper on the shadow of Jupiter, and Jupiter's fourth satellite.

Captain Noble. If I understand Mr. Ranyard's paper rightly, his idea is that the shadow of Jupiter instead of being elliptical has a kind of crenulated outline, on account of the masses of cloud in Jupiter's atmosphere. I can quite see that, assuming that to be the fact, it would decidedly account for the appearance I observed. I saw the satellite almost fade away, and then suddenly it came out brightly and then faded again. Mr. Ranyard's theory would certainly account for what I saw.

The following papers were also announced:—

S. W. Burnham. "Measures of the companion to Sirius."

Prof. G. W. Hough. "Observations of the companion to Sirius made at the Dearborn Observatory, Chicago."

J. Tebbutt. "Post-perihelion observations of the great Comet (b) 1882."

E. de Gothard. "Physical Observations of Jupiter and Mars made during the year 1882 at the Herény Observatory, Hungary."

E. de Gothard. "Astrophysical Observations made during the year 1882 at the Herény Observatory, Hungary."

Prof. J. C. Adams. "Observations of Comet c 1881 (Schäberle)"

made at the Cambridge Observatory with the Northumberland Equatoreal and Square-bar Micrometer."

Prof. J. C. Adams. "Observations of Comet *a* 1883 (Brooks-Swift) made at the Cambridge Observatory with the Northumberland Equatoreal and Square-bar Micrometer."

Prof. J. C. Adams. "Note on Dr. Morrison's paper on the computation of the Eccentric Anomaly &c."

E. E. Markwick. "Observations of Sun-spots from May 1877 to February 1883."

Prof. S. Newcomb. "On Hell's alleged falsification of his observations of the Transit of Venus in 1769."

W. E. Plummer. "On the Solar Motion in Space."

E. J. Stone. "An explanation of the principal cause of the large errors at present existing between the positions of the Moon, deduced from Hansen's Tables and Observations; and the Cause of an apparent increase in the secular acceleration in the Moon's mean motion required by the tables, or of an apparent change in the time of the Earth's rotation."

R. L. J. Ellery. "Note on Photographs of κ Crucis and of the Nebula in Orion."

A. A. Common. "The Nucleus of Comet *b* 1882."

The following gentlemen were duly elected Fellows of the Society:—James Lawson, Emanuel Ristori, and W. G. Woollcombe.

The following distinguished astronomers were proposed as Foreign Associates of the Society:—Dr. J. A. C. Oudemans, Prof. S. P. Langley, Prof. P. Tacchini, and Dr. E. Weiss.

The Meeting adjourned at 10 P.M.

Distribution of the Variable Stars.—Classes II. & III.

I HAVE now to enter upon the task of examining the second class of Variable Stars. These we have said to consist of long-period variables. They form a great zone all round the heavens, inclined some 15° or 20° to the equator. Very little is known about the periods or amount of variation, so that at the very outset there is a difficulty from want of conclusive observations, of periods, &c. But at present our information is sufficient to lead to several remarkable conclusions, which may be valuable, though only approaching to the truth. The shortest period for the variables of Class II. is 135 days, the longest 570. The variation in magnitude is from 1.0 to 9.0; but in the cases where there is very little variation, for reasons set down in my former paper ('Observatory,' March 1882), the period is probably incorrect, or the variation not real, or the stars belong to another class hereafter to be mentioned.

The following table gives the number of stars in relation to the period of variation:—

Period.	Stars.	Period.	Stars.
135-170	7	320-370	21
170-220	9	370-420	15
220-270	10	420-470	7
270-320	15	470-520	3

while the second table gives the number of stars in relation to the variation in magnitude :—

Var. mag.	Stars.	Var. mag.	Stars.
1	3	5	27
2	3	6	25
3	6	7	6
4	14	8 & 9	3

from which we find that

(a) The number of stars increases with the length of the period.

(b) The number of stars increases with the variation in magnitude.

It must, however, remain an open question at present whether the first of these conclusions holds good further than periods of 420 days, or the second in cases of variation beyond five or six magnitudes. I am inclined, however, to think that the second conclusion will only hold good up to five or six magnitudes. Condensing these two tables we get the following :—

From Table I.		From Table II.	
Period. Days.	Stars.	Var. mag.	Stars.
135-270	26	1- 4	12
270-420	51	4- 7	66
420-570	10	7-10	8

Of course we must remember that the determinations of variation in magnitude of all these stars are liable to two sources of error, one in the actual observation, and the other one to the unequal brightness of maxima and also of minima. And this remark applies also to the determination of period ; but I do not think that the errors will be sufficient to vitiate the foregoing conclusions.

On closer examination of the tables I find, then, that

(c) More than two thirds of the variable stars of Class II. vary more than four, and less than six and a half magnitudes.

(d) Nearly two thirds of the variable stars of Class II. have periods between 320 and 420 days.

As regards the relations between the periods and the variation, the results would seem to be negative. The average period does not alter with the variation, as in the case of the variable stars of Class I. The following tables will explain this :—

Table III.	
Period.	Average Variation.
Days.	Magnitudes of Variation.
135-270	4.4
270-420	5.7
420-570	4.8

Table IV.	
Average Periods.	
Magnitudes of Variation.	Period
1 & 2	292
3 & 4	317
5 & 6	294

So that the average variation is almost the same for all periods, while, conversely, the average period is almost the same for all variations of magnitude. In the latter case it must, however, be remarked that after six magnitudes of variation the period lengthens, so that from 7-9 magnitudes the average period is 438 days. The stars with so great a variation are, however, so few, that it is impossible at present to say whether this lengthening of period is real or apparent.

When, nearly eighteen months ago, I commenced the first of these papers, I believed that all cases of stellar variation might, with the exception of temporary stars, be included in Classes I. or II. Lately, however, I have become aware of the existence of a new class, stars which have a small fluctuation in magnitude once in several years. The observations of 63 Cygni (MN. of R. A. S. vol. xliii. p. 271) first led me to this conclusion, and some of the stars suspected of variation now under observation go far to confirm it. For the greater part of the time the light of these stars is constant, but then it alters a magnitude or so, but after a month or two it returns to its ordinary magnitude. Many of the stars suspected of variation undoubtedly belong to this Class III., and it is obvious that only long and careful determinations of magnitude during many years can determine the periods and variation of such stars.

Summing up our results, then, we find four classes of variable stars :—

Class I. With short period and small variation.

Class II. Long period and great variation.

Class III. Period of several years and small variation.

Class IV. Temporary stars.

T. E. ESPIN, B.A., F.R.A.S.,

Vice-President of the Liverpool Astronomical Society.

Uranus *.

I SHOULD like to remind those astronomers who are in possession of large telescopes provided with a complete micrometer-apparatus, that the planet Uranus is just now favourably placed for the accurate investigation of its figure. The plane of the orbits of the satellites is nearly perpendicular to the ecliptic; according to analogy it is to be presumed that the planet's equator is in the same plane, and that we see the profile of its ellipsoid in its true position with regard to the axis only when the Earth is in that plane. Newcomb gives $165^{\circ} 30'$ as the place of the ascending node of the orbits of the satellites; Uranus passed this point in December 1881: at present his heliocentric longitude is $171^{\circ} 36'$, so that his position is still very favourable; but this will soon cease to be the case, after which forty years will elapse before

* Astr. Nach. No. 2505. (Translation.)

he will reach the descending node, so that it is very desirable that the present opportunity should not be lost.

W. Herschel saw Uranus round in 1782, strongly elliptical in 1792 and 1794; Mädler, with the Dorpat refractor, in 1842 and 1843 measured the compression with results $\frac{1}{11}$ and $\frac{1}{19}$; Vogel, in 1871, saw the planet perfectly round; Newcomb, in 1874-75, assigns nothing with regard to the shape of the disk, so that the amount of ellipticity would seem to be not striking. Compare Kaiser (on Planet-diameters, Leyden 'Annals,' iii. pp. 269-271) and Arago ('Analysis of the Works of Sir W. Herschel,' 1842, pp. 577-579).

I have in the last few years often examined the planet very accurately, and made some drawings merely to show that the compression of the disk as seen at present, even with very moderate optical powers (achromatic 11 centimètres, silvered-glass speculum 16 centimètres), is striking, and may therefore in fact be considerable.

1877, March 12. Power 160. Contour not quite sharp; disk of a dark bluish-grey colour. Certainly elliptical; major axis in the parallel. [The observation made a good distance from the meridian and without wire cross.]

1879, May 23. Power 160. Bluish grey; decidedly elliptical disk; major axis in π 200°.

1879, May 23. Power 300. Still decidedly elliptical, in π 200°.

1881, April 19. Power 290. Sharp, pale bluish grey; decidedly elongated, in π 190°.

1882, April 21. Power 250. Tremulous, diffused, greyish blue; repeatedly appeared to me at last moments elongated, in π 225°.

1883, April 1. Power 220. Tolerably sharp, not quite steady, but very bright, beautifully light greyish blue. In all the most distinct moments seemed, without doubt, strongly elliptical, in π 190°-200°, much more strongly so than Jupiter (examined the same evening with low power). Speculum freshly centred; eyepiece focussed on Leonis, and turned round each time by 30° or 40°.

1883, April 2, 12^h. Power 300. As yesterday. Image sharp; somewhat hazy and not sufficient light. Uranus of a remarkably dark blue, in the better moments strikingly elongated in π 200°, more strongly than, or, at any rate, even as strongly as, Saturn without its ring.

1883, April 2, 12^h 40^m. Cloud having sunk, Uranus very bright; let it pass quite thirty times through the field; always appeared strongly elongated; at the best moments I estimated the ellipticity as greater than that of Saturn. π 190°.

Prague, 1883, April 3.

A. SAFARIK.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Apparent Inequality in the Mean Motion of the Moon.

SIR,—

My friend, the President of the Royal Astronomical Society, has offered, for explanation of the recognized inequality in the Mean Motion of the Moon, a theory, connecting the computed motion of the Moon with the computed motion of the Sun, and basing the apparent inequality of the Moon's motion upon a change in the adopted numerical elements of the Sun's motion.

My own examination of the connexion of these movements has led me to a different conclusion, which I desire to place before astronomers. It is founded on the simplest consideration of the methods universally adopted, on the one hand for observing the place of the Moon in the sky, and on the other hand for computing the corresponding tabular place of the Moon.

For simplicity of language, I shall allude only to right ascensions, omitting longitudes. The means of disturbances of the values of these ordinates taken for the mean of all positions on the globe (excepting only positions very near the pole) are sensibly equal.

First, in regard to observation of the Moon's place in the sky. The time of transit of the Moon over the meridian, as shown by the transit-clock, is observed, the time of transit of a star contained in a standard list, as shown by the same transit-clock, is observed; the difference between these transit-times is applied to the tabular R.A. of the star; and thus the adopted R.A. of the Moon is formed. This process is usually extended, but without alteration of principle, by employing several stars in connexion with the same observation of the Moon. And the comparability of the successive inferred R.A.'s of the Moon through a long series of years will depend solely on the consistency of the tabulated R.A.'s of the stars (employed in the reduction above mentioned) through that series of years.

Now the assumed R.A.'s of the clock-stars employed in the great Greenwich Reductions of the Lunar Observations from 1750 to 1830 are taken 'exclusively from Bessel's *'Tabulæ Regiomontanæ.'* This noble work was continued with unaltered elements by Wolfers. The star-places subsequently employed at Greenwich were taken from Greenwich lists, from a list prepared by Professor Adams, and from the Greenwich 7-year and 9-year Catalogues, and, though Wolfers's Catalogue has not been expressly used, comparison with it has shown that the elements employed are sensibly the same (the greatest discordance for a single star is $0^{\circ}.08$). Thus we can assert that the whole of this great series of Lunar observations, and the places of the Moon at these observations, are reduced by a consistent catalogue of star-places, nowhere depending on Solar Tables.

Secondly, the computation of the Tabular Places of the Moon, with which the places inferred from observation have been compared, is effected by conversion of the Sidereal Time at each observation of the Moon into the corresponding Mean Solar Time. This conversion does imply, theoretically, a dependence on the accuracy of the Solar Tables; but, practically, no sensible error in the computed place of the Moon can be inferred from possible inaccuracy of those Tables. Nobody supposes that the Mean Solar Time is in error to the amount of 1^s of time; and this would produce in the computed place of the Moon an error not exceeding 0".5 of arc, a quantity of no importance in the present inquiry.

I conceive, therefore, that it may be accepted as established that no admissible error in the tabular places of the Sun or the principal stars can explain the discordance of Lunar results which has led to this investigation.

G. B. AIRY.

The White House, Greenwich,
1883, May 15.

Saturn's Ring.

SIR,—

To use a phrase which has become proverbial, "many things have happened" since I last wrote to you on this subject, and I would crave space for a few words with reference to some of them.

That Cassini was the first discoverer of the principal division in Saturn's ring, and that the supposition that our countryman William Ball anticipated him in this is founded on a mistake, are points now universally admitted, and respecting which there is no doubt. But subsidiary questions have been raised regarding the accuracy of the engraving purporting to be a representation of a drawing made by Ball on the 13th of October, 1665, which is found in a few, and only a few, copies of the first volume of the 'Philosophical Transactions.'

In the January number of our 'Monthly Notices' is a paper by Professor Adams, in which he states that the engraving was made from a paper cutting, which he thinks was folded twice in directions at right angles to each other; he then suggests that only one quadrant of the ellipse representing the planet's ring was cut out, and the lines of folding not exactly corresponding with the two axes of the ellipse, the result was that when the whole was opened out, the figure appeared to have those curious shallow notches or depressions at the extremities of the minor axis which are shown in the engraving (see 'The Observatory' for last November, Vol. v. p. 332). At first I felt inclined, unreservedly, to accept this ingenious explanation, but on subsequent consideration I confess I feel much more doubtful about doing so. In the first place, Ball himself says that the figure was "a little hollow above and below." Professor Adams thinks that "this clause has been added or altered in some way to correspond with the given figure." This is, of course, pure conjecture, and it is surely too much to

suppose that Sir Robert Moray would so alter, or add to, what professes to be a quotation from Ball's own letter. It is true that Prof. Adams adduces a small sketch (with description) of Saturn as observed by Ball in April 1666, which shows no trace of any such notches or depressions as those depicted in the previous October, and is unaccompanied by any remark about a different appearance then presented. But surely this would be sufficiently accounted for by the supposition that the engraver considerably exaggerated the hollownesses of which Ball may have thought he saw indications in October 1665. The drawing in April 1666 is too small and rough for any decided opinion about the total absence of such even then. Ball at the later time had not seen the engraving of his former drawing; and I think his language *does* seem to imply some change in the apparent figure of the ring:—"what his figure was last autumn I suppose Sir R. Moray hath communicated." Prof. Adams supposes that the erroneous figure in the engraving was soon discovered, and that an effort was made to conceal it by cutting out the plate containing it, re-engraving in a subsequent plate the only other two figures on the one in question; this, he thinks, accounts for the plate with the erroneous figure being found in a few copies only of the 'Transactions.' But surely, if this were so, it would have been a very strange way of cancelling a plate, simply to cut it out of all the copies within reach, without a word of explanation to account for the erroneous description still remaining, which Prof. Adams thinks was written, not by Ball, but by some one inexperienced in astronomical observations, for the express purpose of corresponding with the figure. Moreover, I must remark that Prof. Adams seems to have overlooked that the repetition of the other two figures in the plate (of machines for drawing water from the depths of the sea) is quite sufficiently accounted for by a *second reference to them* in the later number. At the same time their insertion then may have led to the cutting out of the earlier plate in a few copies, as being superfluous. Having looked, during this inquiry, at a very large number of copies, I may be allowed to state that several courses seem to have been adopted. In some cases the first plate does seem to have been cut out; in others it would seem that it had never been inserted; and in yet others the second plate has been inserted twice. Let it never be forgotten that, in consequence of the plague and the fire of London, there was great difficulty in printing and binding the numbers of the 'Philosophical Transactions' for the first two years, 1665 and 1666.

In the March number of the 'Monthly Notices' Mr. Vivian suggests that Sir Robert Moray's remarks and request to Huygens to examine Saturn again were meant as an attempt (induced by the representation of Ball's drawing of October 13th, 1665) to revive the old theory of Galilei and others, that Saturn was elliptical and enclosed between two circular-shaped bodies, one on each side of him; this view, I think, cannot possibly be entertained.

Sir Robert's language is quite clear that he accepts the theory of a ring or body quite surrounding Saturn ; but he suggests that there were two *such* bodies, not one.

Whilst still full of conjectures on this subject, I received a letter from Mr. Prince of Crowborough, calling my attention (for a different purpose) to a passage he had met with in Evelyn's Diary, which, it has occurred to me, may throw some light upon this subject, as showing how these matters appeared to a man of education of that time, who was not specially versed in astronomy—the position of Moray as well as Evelyn. The passage is as follows, dated May 3, 1661 :—" This evening I was with my Lord Brouncker, Sir Robert Murray" [of course another way of spelling Moray], " Sir Patrick Neill, *Monsieur Zulichem*" [*i.e.* Huygens], "and Bull*" (all of them of our Society, and excellent mathematicians), to show his Majesty, who was present, Saturn's annulus, as some thought, but as Zulichem affirmed with his *balteus* (as that learned gentleman had published) very near eclipsed by the Moon." At the first reading of this passage we are surprised to seem to see it implied that Huygens, who discovered that the appendage to Saturn, so long a puzzle to astronomers, was, in fact, a ring or annulus surrounding the planet, affirmed that it was something else, a *balteus* or belt. But I think a little consideration will remove this surprise, and also explain the difficulty with which we are concerned. Evelyn, not quite comprehending the whole subject, heard Huygens explain how he had recently proved that Saturn's appendage was a ring, by noticing it at certain times appear like a dark belt passing nearly over the centre of the planet. Now Sir Robert Moray (according to my hypothesis), seeing in Ball's drawing, made about $4\frac{1}{2}$ years after this conversation, that the appendage appeared to pass, not over the centre, but over the top and bottom of the planet, and not appreciating how this would be due to the different position of Saturn with respect to the Sun and Earth, thought that there were two such rings, the one which Huygens had described as resembling when he saw it a belt, and the one represented by Ball, which, for some unexplained reason, was not visible at the time of the observation of Huygens ; so he, in fact, wrote to the latter to ask him to look attentively, and see whether, on close scrutiny, both these presumed rings were visible at once. It would be interesting indeed if Huygens's answer, supposing it to exist, could be found ; but I fear there is little hope of this. In conclusion, let me ask those of your readers who are interested in this subject to give my present conjecture their thoughtful consideration.

Yours faithfully,

W. T. LYNN.

Blackheath, 1883, May 5.

* It was with reference to this name that Mr. Prince wrote to me ; I feel sure it is merely a misprint for Ball, especially as there is no such name as Bull in the early lists of the Royal Society.

Bergman's Observation of the Transit of Venus in 1761.

SIR,—

At the last two transits of Venus, in 1874 and 1882, several observers (amongst them Mr. Prince of Crowborough*) noticed that the outer portion of the limb of the planet when she was partly on the Sun's disk was illuminated, so as to cause her to present the appearance of a complete circle, an effect in all probability due to the refractive power of the planet's atmosphere.

Prof. Newcomb states, in his truly valuable work on 'Popular Astronomy,' that this appearance "was first noticed by David Rittenhouse of Philadelphia, while observing the transit of Venus on June 3rd, 1769." I was rather surprised to find no note or remark on the passage in the German edition of Prof. Newcomb's book, published by Dr. Engelmann at Leipzig in 1881, nor is it corrected in the author's second edition, which has just appeared. I conclude, therefore, that some of the observations made of the transit in 1761 have been strangely overlooked; and beg to call your attention to the account of Mr. Thorman Bergman of Upsala, read before the Royal Society on November 19, 1761, and published in the 52nd volume of the 'Philosophical Transactions,' part 1. p. [227].

As your readers will be able to refer to the original account and the drawings by which it is accompanied, it will suffice here to quote the following portion:—

"Imprimis, Venerem atmosphæra circumdatam observasse credimus; sequentibus nixi rationibus. Scilicet, ante completam immersionem, seu adhuc quarta circiter parte diametri Veneris extra marginem Solis existente, tota Venus visa est; nam pars extra prominens debili lumine erat cincta, uti Fig. 1 monstat (Plate XLVIII.). Hoc vero longe clarius sub emersione notatum fuit, &c."

Whilst on this subject, it may be worth while to refer to another omission with regard to a perception of a similar kind. It is known that when Venus is near her mode at the time of inferior conjunction, the entire circle of her disk has sometimes been seen, an exceedingly fine thread of light being stretched round the side of the planet furthest from the Sun. Prof. Newcomb states that this was first noticed by Prof. C. S. Lyman of Yale College, in the month of December 1866. But a reference to our 'Monthly Notices,' Vol. xxiv. p. 25, will show that it was seen by Mr. Prince at Uckfield near the time of the inferior conjunction, which occurred towards the end of September 1863, more than three years before Prof. Lyman's observation.

Hoping you will think these particulars of sufficient interest to print in your next number,

I remain, yours faithfully,

W. T. LYNN.

Blackheath, 1883, May 2.

* See his pamphlet on the Great Comet and the Transit of Venus, noticed in your last Number, p. 160.

Relative Heights of Markings on Jupiter.

SIR,—

I should like to make a few remarks on Mr. N. E. Green's letter in the 'Observatory' for April on this subject; and first a word or two on the value which ought to be set on drawings of Jupiter in determining a question of this kind, which is one of considerable delicacy. Without wishing to disparage them more than is needful, for some of them are certainly marvels of accuracy, it must, I think, be admitted that the drawings in general fall far short of the necessary exactness. In the first place, the amount of detail is so immense, and so highly complex in nature, that some considerable time would have to be employed in making a truthful representation of even a small portion of the disk. —This time, however, is not permitted, for the rotation of Jupiter is so rapid that a quarter of an hour will generally completely change the appearance of the planet, even in the parts near the centre of the disk. A drawing, then, of the whole disk, and nearly all are of this nature, to show detail sufficient for our present purpose must necessarily have been more or less hurriedly, and therefore inaccurately, delineated. Secondly, the subject of *irradiation* becomes an important one in interpreting the drawings of Jupiter. The white spots and patches from their brilliancy would generally be among the first objects to attract attention; and, owing to irradiation, these would not only be drawn larger than their real size, but also frequently projecting into, or apparently over, a dark belt or other feature, in the same way as the polar caps of Mars appear to project beyond the disk of that planet; and it may be remarked that the contrast between some of the brilliant white spots and patches on Jupiter and a dark belt is frequently greater than that between the caps of Mars and the sky around. The greater brightness and prominence of the white markings also give them the appearance of being nearer than the dark ones, and together with the fact of the general acceptance that the light markings are situate above the dark ones, must also have exercised some influence upon the draftsman. These and other reasons seem to show that evidence derived from drawings should be rejected, except in extremely well-marked cases. An examination of nearly 300 drawings thus appears to furnish little decided evidence in favour of the white markings being above the dark ones, whilst a considerable number of fairly well-marked cases point to the contrary. By far the greater number, however, afford no certain evidence either way. Now, taking Mr. Green's statements in the order there given, —

First, as to the general form of the light markings. It is only the very small white spots which can be said to be "round, oval, or compact patches." The larger ones are nearly invariably irregular in outline, and without defined edges, except where they touch upon a dark belt or other marking. The dark spots, moreover, are usually of the same form and at least equally compact.

Second. I have never been able to see one of the oval forms indenting *equally* the dark belt and the general surface of the planet, but always the feature has been sharply bounded by the dark belt, and on the other sides more or less gradually merged in the dusky surface round, and generally in a highly complex and irregular manner. Here the drawings seem to afford strong support, and in particular some of those made by Mr. Gledhill in 1869-71 may be mentioned. Such cases may occur, but must be very rare; their apparent indication in some of the drawings may be ascribed to an intention to give greater prominence to the spots, for without some provision of this nature it is frequently quite impossible to do remote justice to the dazzling and conspicuous character of some of the light markings. The apparent indentation into a dark belt has been already accounted for by the well-known phenomena of "projection," and is considerably diminished by intense gazing.

Third. On the supposition of the dark belts being situate above the light surface, a break in one would indicate a great want of the matter composing it at that place; and this deficiency would also probably extend for some distance around, enabling the lighter surface to be seen, so that to produce the observed appearances it would be unnecessary to imagine a white patch suspended in part over the belt.

It does not seem much harder to believe the strings of white spots seen in 1874 on the dark belts were caused by gaps or holes in the belt itself, than that a row of actual small bodies was suspended over it. Strings of *dark* spots have often been seen on the dark belts, and it is not difficult to imagine, under somewhat different circumstances, rows of small holes. With regard to the southern belt, in Jan. 1873, looking like a bridge by reason of many large oval masses of light, much the same thing is shown in many of Mr. Gledhill's drawings; but, as already pointed out, the southern sides of the oval white markings next the belt are more or less sharply and *straightly* bounded, whilst on the north they are generally rounded, ill-defined, and irregular in outline, which is strong evidence in favour of the dark belt sharply cutting off the light mass on the south, which the deficiency of matter on the north prevented doing, except in a gradual and partial manner.

At the present time the great southern dark belt may be said to be triple, consisting of (1) a central dark well-defined band, visible for several years past, and which I have always spoken of as the south equatoreal dark belt; (2) a belt now nearly equally broad and plain, situate immediately south of the first, and which has chiefly been formed during the past year; (3) a belt formed of disconnected dark spots and streaks, lying just north of the preceding two. The spots and streaks of this third belt are the darkest portions of the great compound southern band; and if Mr. Green's observation of February 18 refers to this belt it would be due to the disconnected character of this feature, the past history of which is not favourable to the supposition of its discontinuous

character being caused by overlying light masses. I hope to be pardoned for expressing the opinion that the observation of Feb. 24 could be at least as well accounted for by the disruption of the bluish streak, combined with the effects of irradiation; and the fact that portions of the belt were seen north and south of the square light patch favours this view.

The following are a few instances which appear to indicate that the dark features lie uppermost:—1881, Sept. 28, 12^h 55^m. A large ill-defined whitish patch, which apparently penetrated under the south component of the double N. equatoreal dark belt, and was visible in the rift between it and the northern component.—1882, Sept. 21, 16^h 23^m. An excessively bright, white equatoreal spot of irregular form. Its southern side (in length nearly equal to the whole breadth of the equatoreal zone) was sharply and straightly bounded by the south equatoreal belt, and its north-west corner was cut sharply off by a very dark well-defined patch on the north equatoreal belt.—Nov. 20, 14^h 56^m. An exceedingly bright, large, very well-defined white spot, surrounded with radiation of lesser brilliancy, which penetrated beyond one of the narrow lines into which the north equatoreal belt had broken up, and without altering its character.—Dec. 23, 9^h 59^m. A large, nearly round, very bright white spot traversed and cut into two unequal portions by one of the dark lines composing the north equatoreal belt. These are only a few of the instances which have been noted, but it would be premature to give them more fully before the reduction of the transit observations. I have never noticed what seemed to me a conclusive instance of a dark feature being obscured by a light one during the last 3 or 4 years; but it is of course possible that these do occur, especially in the much disturbed region between the two equatoreal dark zones.

Yours faithfully,

A. STANLEY WILLIAMS.

West Brighton,
23rd April, 1883.

19 *Leporis*.

SIR,—

With reference to Mr. Tebbutt's remarks about this star in the 'Observatory' for May 1883, I may say that the star is certainly not a "known variable." I have lately compiled from various sources a catalogue of over 500 "suspected variable stars," and 19 *Leporis* is not among them. If, however, there is any reliance to be placed on the 4 m. of the Cape Catalogue, the star must be variable. It is 6 m. in Heis's Catalogue, but is not in Argelander's 'Uranometria.'

I am, Sir, yours faithfully,

J. E. GORE.

Ballisodare, Co. Sligo,
1883, May 8.

NOTES.

THE AURORAL BEAM OF 1882, NOVEMBER 17*.—The month of November 1882 was rendered remarkable by the appearance of the largest sun-spot seen during the last eleven years, and by the occurrence of a series of remarkable auroral and magnetic storms, in precise coincidence with the period during which the spot was visible. But the most unique and striking phenomenon of this period occurred during the great auroral storm of November 17 at about 6 P.M., when a bright beam of light rose from the eastern horizon and passed majestically across the sky in much the same manner as any ordinary celestial body might do, but with several hundred times their rapidity. So beautiful and wonderful an appearance deserved that the fullest possible attention should be paid to it; and with this view Mr. Rand Capron has collected as many observations of it as he could procure, and has discussed them in an interesting paper appearing in the 'Philosophical Magazine' for May.

Mr. Capron has tabulated some twenty-six observations of the phenomenon, of which all but five were made in England. Most of these are incomplete in many particulars which we could wish had been remarked; and the exactitude of the observations actually made leaves much to be desired; but the phenomenon appeared so suddenly, disappeared so soon, and was altogether so unusual and striking, not to say awe-inspiring, that it is rather satisfactory to find so much recorded than surprising that more information has not been secured. Mr. Capron's own spectroscopic observations, for instance, satisfactorily establish the true auroral character of the object, against the theory warmly advocated by several observers that it was a "meteor" or "meteoroid;" and Mr. Capron sums up other principal particulars as follows:—"Time of appearance, a little after 6 P.M. Time of flight, about 75 seconds. Apparent approximate length and breadth, $27^{\circ} \times 3\frac{1}{3}^{\circ}$. Direction of flight, magnetic E. to W."

The shape of the beam is described in very various language; "cigar" or "spindle"-shaped represents pretty accurately the opinion of most. "Pearly white" or "greenish white" are amongst the descriptions of the colour of its light, which seems to have closely corresponded to the "citron" tint of the auroral line (W.L. 5569); and as to its quality, the comparison to an "electric glow *in vacuo*" gives as accurate an idea as any. To most English observers it seemed pretty well defined in outline; but some noticed a feathery or splintered appearance about its edges. Prof. Oudemans noticed a remarkable dark marking, 10° in length, down the centre (and his observation is confirmed by observers at Brussels and Chatham), as if the beam had really the shape of "a pulled-out ring," "what one might anticipate of an elastic gaseous or

* Phil. Mag. 1883, May.

fluid-like body flying at considerable speed through a resisting medium."

The most interesting point, and the one to which a considerable part of the paper is devoted, is the question of the height of the object; and Mr. Capron fixes this, with considerable probability, as about 133 miles. From this height it would follow that its rate of motion was about 10 miles per second. This rate of motion is so much slower than the average speed of bolides, as to strengthen the conclusion drawn from the spectroscopic observations, that the phenomenon was truly auroral, and not of a "meteoric" character, a conclusion still further strengthened by the direction of its flight, viz., as nearly as can be ascertained, from magnetic E. to W.

With regard to the character of the beam, Mr. Capron quotes Prof. Herschel, who "has seen abortive bright streamers move along stationary milk-white auroral bands; and this 'shuttle phenomenon' was, he thinks, a streamer-base or 'tendency to shoot up rays,' travelling as a nucleus or concentration of light along an arc or bow otherwise invisible, as streamer-bases, though more faintly visible, frequently do along luminous sharp-edged phosphorescent-looking arches." Certainly our own impression at the time was in complete accordance with this view, "that the beam itself may have been the transient lighting-up by a passing glow of an otherwise invisible arc."

E. W. M.

KEILL AND THE STAR OF 1572.—It is pointed out in 'Nature' for May 17, that in all probability the idea that Keill believed that this star had a period of 150 years, instead of about 300, as Leowitz had imagined, arose from a misunderstanding of Keill's language. It seems evident, that what Keill meant was that this star might appear again in 150 years from the time when he was writing, which happened to be about 150 years after Tycho's observation. The clause about the 150 years, and which has apparently been misunderstood, was added by Keill in the English edition of his 'Introduction to the Astronomy.' In the original Latin edition (published in 1725), he simply says, "*credibile est hanc*" (i. e. the so-called star of 1264) "*et supra memoratam quæ anno 945 apparuit eandem fuisse stellam cum eâ quæ a Tychone visa fuit.*" The person who first affixed an erroneous meaning to the clause added in the English edition ("and that in about 150 years the same *star* may again make its appearance") seems to have been E. Pigott (son of N. Pigott), referred to in the 'Observatory,' No. 73, p. 151. He says, speaking of this star, "several astronomers are of opinion that it has a periodical return, which Keill and others have conjectured to happen every 150 years." W. T. L.

THE TOTAL SOLAR ECLIPSE OF 1883, MAY 6.—The English and American observers who have gone out to observe this eclipse left Callao for Caroline Island on March 24, in the U.S. steamship 'Hartford.' Nearly a month more must elapse before we can hear

whether they have met with good fortune or not ; but every preparation had been made that the greatest possible number of good observations might be secured if the weather proved favourable. The English observers have confined their work entirely to photography, and their programme included the taking of over sixty photographs with very widely different exposures, so that some at all events might turn out well. One of the Dallmeyer photoheliographs used in the 1874 Transit of Venus has been taken out on the expedition, in order to obtain photographs of the corona on a scale larger than has yet been attempted, viz. four inches to the solar diameter. Three photographs were to be taken with this instrument, the exposures varying from 50 secs. to 150 secs. Another camera for photographing the corona was to take six pictures, with exposures varying from 1 sec. to 2 mins. The cameras to be used with the siderostat and with the equatoreal were all designed for photographs of the spectrum. Of these, one of the most important is attached to an integrating spectroscope, and has a clockwork arrangement by which the sensitive plate could be kept slowly and regularly moving, so that if any changes took place in the spectrum during the period of totality, they might be recorded ; and since the time when the plate was first exposed and when it was again covered would be known, the time when the changes took place could also be determined. All the other cameras are provided with long plates, of which only a portion is exposed at any one time, fresh portions of the plate being brought into position by turning a screw. Amongst the spectroscopes is a beautiful Rowland grating of 10 feet focus, and ruled with 14,000 lines to the inch, presented by Prof. Rowland to Mr. Norman Lockyer. In this grating, which will be used in connection with the siderostat, and in another, a flat grating mounted on the equatoreal, both the spectra of the first and second order are to be employed. Two other instruments, viz. a prismatic camera and slit spectroscope of two prisms, were employed in the eclipse expedition to Egypt last year. The English observers, Messrs. Woods and Lawrance, were to attend to the siderostat and equatoreals and their connected instruments, whilst some officers from the 'Hartford' would take charge of the photoheliographs and the eclipse clock.

PROF. YOUNG'S OBSERVATIONS OF THE TRANSIT OF VENUS.—The 'American Journal of Science' for May 1883 contains Prof. Young's report of the observations of the Transit of Venus made at Princeton, New Jersey, and at South Hadley, Mass. Ten observers watched the phenomenon at Princeton, besides eleven students, who observed images of the Sun thrown upon screens by a 9-inch Newtonian and a 5½-inch comet-seeker. The larger instruments, viz. the great 23-inch equatoreal of the Halstead Observatory and the 9½-inch equatoreal of the School of Science Observatory, were stopped down to 5¾ inches at the two internal contacts, that the

observations might be comparable, so far as possible, with those of the various government expeditions. The full aperture was used for the first external contact with the $9\frac{1}{2}$ and for both with the 23-inch. All four contacts were secured at Princeton, though clouds passed very frequently and proved very troublesome at the first and third contacts; at South Hadley only the two last contacts could be observed. Prof. Young also made two sets of micrometric measures of the planet's diameter, one set with a double-image micrometer, the other with a filar micrometer. The double-image gave for the observed diameter $62''.11$, corresponding at mean distance to $16''.43$; the filar micrometer gave $64''.23$, corresponding to $16''.99$ at mean distance. 191 photographs were taken by means of a photoheliograph of similar construction to those used in the U.S. Government expeditions.

The presence of an atmosphere round Venus was so unmistakably demonstrated by the ring of light seen round the planet when still only half on the disk of the Sun, that it seems a little strange that the spectroscope had not a very distinct evidence to give. B and α were unaffected, nor could Prof. Young detect any change certainly due to the planet's atmosphere between C and D. "*But the vapour-lines just below D_1 and between D_1 and D_2 were unmistakably intensified close to the planet's limb.*" Prof. Young's observation was confirmed by Mr. McNeill at the $9\frac{1}{2}$ -inch telescope, the latter observer also remarking the intensification of a fine line near C ($\lambda=6392$). These observations, both as to *general* absence of the spectroscopic indications of an atmosphere, and also as to the slight changes noted near D, were closely confirmed by the observations made by M. Janssen in Algeria, in an atmosphere so clear that he was able to use a power of 150 with advantage on the craters illuminated by earth-shine in a four days old Moon. M. Janssen seems, however, to have had greater difficulty in detecting any trace of an atmosphere even than Prof. Young had, and thinks that the presence of water vapour in the atmosphere of Venus has been asserted more strongly than the proofs hitherto possessed quite warranted.

THE OBSERVATORY OF MOSCOW.—The first part of the ninth volume of the 'Annales de l'Observatoire de Moscou' has recently been issued. The principal portion of it is occupied by the observations of Victoria made by M. Socoloff during the opposition of 1882, in connection with Dr. Gill's scheme for a new determination of the solar parallax from observations of the minor planets. Amongst the other contents are papers by Dr. Th. Bredichin on the resisting medium, and on Comet 1882 α (Wells), the latter paper, which has special reference to the form of the tail of the comet, as indicated by theory and as actually observed, being illustrated by four plates. M. Bredichin criticises severely M. Schwedoff's "theory of cosmic waves" in which the latter holds that the forms of comets' tails are identical with those which would be presented by the waves produced by the nucleus in a resisting medium. M. Schwedoff

has since claimed * that the form of the Great September Comet, 1882 *b*, is in strict accordance with his theory.

SYSTEMATIC SEARCH FOR COMETS†.—The ‘Science Observer’ reports that the work of the observers who have been searching for comets, in accordance with the plan proposed by the ‘Science Observer,’ has been productive of good results. The records for September, October, and November given herewith are creditable in the extreme. The gentlemen who have taken the larger zones have faithfully searched, and the amount of this work, of which there was no previous record, is surprising. The following table gives the number of hours spent in sweeping by the principal observers during the months of September, October, and November, 1882:—

	Sept. h.	Oct. h.	Nov. h.
Swift	52	85	5
Brooks	17	48	32
Barnard	35	47	32
Larkin	12	11	13
	116	191	82

During these searches Mr. Barnard discovered a comet on September 17 (comet 1882 *c*), and six or eight comet-like masses, 6° S.W. of comet 1882 *b* (the great September comet), on October 13. Mr. Brooks also found a comet-like mass, 8° E. of the great comet, on October 21.

BLACK TRANSIT OF THE THIRD SATELLITE OF JUPITER‡.—Mr. Barnard of Nashville, Tennessee, gives the following account of his observation of a dark transit of the third satellite of Jupiter:—“I observed, on the night of February 12, at 9^h 40^m, Jupiter’s third satellite transiting the planet as a small very black spot, smaller than its shadow generally is. Its visible path lay about halfway between the south pole and the southern edge of the heavy band just south of the equator. The satellite was watched until midway across, when observation of its progress was discontinued. At this time, IV. was at superior conjunction close above the north pole of the planet. The planet was not looked at again until 12^h 30^m, when III. had left the disk some distance and appeared about as bright as II. and I., certainly as bright as the disk on which it had appeared as a black spot. I witnessed black transits of this moon on two former occasions, viz. August 2, 1879, at about 15^h, and December 30, 1880, at about 8^h.”

MRS. MARY DRAPER, widow of the late Dr. Henry Draper, has presented six thousand dollars to the United States Academy of

* Comptes Rendus, Vol. xvi. No. 19.

† Science Observer, Nos. 41, 42.

‡ Sidereal Messenger, Vol. ii. No. 1.

Sciences, under a trust deed directing that a gold medal, of the value of two hundred dollars, to be called the Henry Draper Medal, should be awarded from time to time, but not oftener than once in two years, to any person who may have made an original investigation in astronomical physics of sufficient importance, in the estimation of the Academy, to merit such reward. If, after providing for the medal, there is a sufficient surplus from the fund, it may be employed in assisting investigations in the same department of science carried on by some American citizen. It is also stated that Mrs. Draper intends to republish in a cheap form some of her husband's most valuable works.

WE learn from 'Nature' that Mr. Common has maintained a thorough search for D'Arrest's comet with his great three-foot reflector, but hitherto without success. He remarks that the number of faint nebulae about the predicted path of the comet is very surprising.

A NEW Minor Planet (No. 233), of the eleventh magnitude, was discovered by M. Borrelly, at Marseille, on May 11.

-Astronomical Memoranda, 1883, June.

Sun. June 1, sets $8^h 4^m$, rises $15^h 51^m$; June 11, sets $8^h 14^m$, rises $15^h 44^m$; June 21, sets $8^h 18^m$, rises $15^h 45^m$; June 30, sets $8^h 18^m$, rises $15^h 49^m$.

Equation of Time:—Sun *before* Clock, June 1, $2^m 29^s$; June 11, $0^m 42^s$; *after* Clock, June 21, $1^m 25^s$; June 30, $3^m 18^s$.

Sidereal Time at Mean Noon:—June 1, $4^h 38^m 5$; June 11, $5^h 17^m 9$; June 21, $5^h 57^m 4$; June 30, $6^h 32^m 8$.

<i>Moon. rises.</i>			<i>sets.</i>			<i>rises.</i>		
	<i>h</i>	<i>m</i>		<i>h</i>	<i>m</i>		<i>h</i>	<i>m</i>
June 1..	14	2	June 11..	11	43	June 21..	8	41
2..	14	38	12..	12	4	22..	9	18
3..	15	20	13..	12	26	23..	9	51
4..	<i>sets.</i>		14..	12	48	24..	10	18
5..	8	19	15..	13	12	25..	10	44
6..	9	11	16..	13	42	26..	11	10
7..	9	53	17..	14	16	27..	11	36
8..	10	28	18..	14	56	28..	12	4
9..	10	56	19..	15	44	29..	12	36
10..	11	20	20..			30..	13	14

New Moon, June 4, $18^h 12^m$; First Quarter, June 12, $2^h 42^m$; Full Moon, June 20, $4^h 32^m$; Last Quarter, June 27, $7^h 38^m$.

The Moon is in conjunction with Mars, June 1, 23^h ; with Venus, June 2, 16^h ; with Saturn, June 3, 21^h ; with Mercury, June 5, 1^h ; with Jupiter, June 6, 9^h ; and with Mars a second time, June 30, 16^h .

Mercury is in inferior conjunction with the Sun, June 7, 16^h , and is stationary, June 19, 14^h . He is a morning star during the latter part of the month, but not favourably placed for observation.

Venus is a morning star, passing from Aries into Taurus about June 10. Diameter:—June 1, $11''\cdot7$; June 30, $10''\cdot7$. Illuminated portion of disk, 0·895 on June 15.

June 1, R.A. $2^h 37^m\cdot3$, Dec. $13^\circ 35' N.$, tr. $22^h 0^m$, rises $14^h 43^m$
 30, 5 0 '3, 21 55 N., 22 29 14 21

Mars is a morning star, passing from Aries into Taurus towards the end of the month. Diameter:—June 1, $5''\cdot4$; June 30, $5''\cdot8$. Illuminated portion of disk, 0·938 on June 15.

June 1, R.A. $1^h 59^m\cdot4$, Dec. $11^\circ 15' N.$, tr. $21^h 20^m$, rises $14^h 17^m$
 30, 3 22 '6, 17 55 N., 20 49 13 7

Saturn is in conjunction with Venus, June 19, 12^h , Venus being $0^\circ 35' N.$ of Saturn.

Uranus is in quadrature with the Sun, June 10, 1^h .

June 1, R.A. $11^h 21^m\cdot6$, Dec. $4^\circ 59' N.$, tr. $6^h 42^m$
 30, 11 23 '4, 4 45 N., 4 50

Neptune is in conjunction with Venus, June 8, 1^h , Venus being $0^\circ 9' N.$, and with Mars, June 26, 16^h .

Occultations.

G. M. T.			G. M. T.		
h	m		h	m	
June 10	9 10 14	Sextantis	June 17	12 0 41	Libræ
		Oc. D. 93° .			Oc. D. 2° .
	10 6 14	Sextantis		12 14 41	Libræ
		Oc. R. 224° .			Oc. R. 342° .
	14 8 54 50	Virginis		20 8 55 16	Sagittarii
		Oc. D. 90° .			Oc. D. 58° .
	10 8 50	Virginis		9 57 16	Sagittarii
		Oc. R. 227° .			Oc. R. 303° .

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

Publications received:—C. L. Prince, *Observations upon the late Great Comet and Transit of Venus, made at Crowborough, Sussex, in the Year 1882* (Wolff, Lewes)—*Science Observer*, Nos. 41, 42 (Boston Scientific Society, Boston)—Henry Harrison, *A Handbook describing objects in the "Telescopic Pictures of the Moon"* (New York)—T. E. Espin, *The Comet of 1882, a History of its Discovery and Appearance* (Trans. of the Liverpool Astronomical Society, No. 1)—*Abstract of Proceedings of the Liverpool Astronomical Society, Third, Fourth, and Fifth Meetings of the Session 1882-83*—C. A. Young, *Observations of the Transit of Venus, Dec. 6, 1882, at Princeton, N. J., and South Hadley, Mass.* (Amer. Journ. of Science, Vol. xxv. May 1883)—E. C. Pickering, *Mountain Observatories*—J. Rand Capron, *The Auroral Beam of November 17, 1882* (Phil. Mag. May 1883)—*Science* (Cambridge, Mass., U.S.A.)—Th. Bredichin, *Annales de l'Observatoire de Moscou*, Vol. ix. livr. 1—*Meteorological Observations made at the Adelaide Observatory in the year 1880* (Government Press, Adelaide).

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 75.

JULY 2.

1883.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1883, June 8.

E. J. STONE, M.A., F.R.S., *President*, in the Chair.

Secretaries: J. W. L. GLAISHER, M.A., F.R.S., and
E. B. KNOBEL.

THE Minutes of the last Meeting were read and confirmed.

Mr. Knobel. Since the last Meeting sixty presents have been received; amongst them a series of drawings by M. de Gothard of Jupiter and Mars, and some photographs of the partial eclipse of the Sun, May 16, 1882, from the Herény Observatory, Hungary.

The President. We have present a very distinguished visitor, Professor Pickering, from the United States. He is well known to us by his works, and needs no introduction to the Society. (Applause.) I understand that Professor Pickering is willing to give us some account of the work on which he has been engaged. (Applause.)

Prof. Pickering. Mr. President and Gentlemen,—It gives me very great pleasure to have the opportunity of thanking you for the distinguished honour you have conferred upon me, in making me a Foreign Associate of this Society; and I think there is no way in which I can express my gratification better than by giving you some little description of some of the work we have been doing at the Harvard College Observatory [Applause], and so showing how I have been endeavouring to uphold this honour by the work I have now in progress. I therefore avail myself, with much pleasure, of the permission your President has accorded me, to give you some description of the following problem—"On the determination of the light and colour of the Stars by Photography." That is really a piece of a larger work on which we have been engaged

for several years. Some four years ago I undertook the determination of the light of the stars by means of a photometer which has already been described to this Society; and I have before me the proof-sheets of a catalogue of 4260 stars, including all those which are assumed to be of the sixth magnitude or brighter in any of the standard catalogues. I have brought it here so that at the close of the meeting it may be examined. The entire work, which was completed last summer (that is to say about a year ago), includes something like 90,000 observations, and a large portion of it is already in print. A reduction has also been made of the principal authorities for stellar magnitude, beginning with the 'Almagest' of Ptolemy, and the catalogue of Al-Sûfi, a thousand years later. We have made a complete discussion of the important observations of Sir William Herschel, which were published nearly a century ago, in the 'Philosophical Transactions,' and long remained unreduced, and they appear to me to form the most valuable contribution to our knowledge of the comparative brightness of the stars which has been made till within the last very few years. The valuable work of Sir John Herschel, the 'Uranometria Nova,' 'Durchmusterung,' and numerous later standard authorities will be represented in the Catalogue, so that it will indicate for each star the amount of the deviation of our photometric observations from those of the other Catalogues, when all are reduced to the same system. In all those cases, however, the light of the star as seen by the eye has been given, and no account has been taken of colour. The only determination of the colours of any large number of stars, of which I am aware, is that made by a member of this Society, Mr. Franks; and I am greatly indebted to this Society for sending me the manuscript copy of that work so that I was enabled to examine it, and to make use of certain of its results, so as to include them with the rest of our work. I think it is a remarkable instance of the advantage of cooperation between different countries that a manuscript of this kind should have thus been sent 3000 miles away, made use of there, and returned. (Hear, hear.)

My attention has recently been directed towards the subject of photography as a means of supplementing these researches; and I may say that in this work my brother, Mr. W. H. Pickering, has been associated with me, and all the direct photographic work has been under his care. The work of planning has been mine; that of photographing, his. The problem we had before us was to compare by photography all the stars which are visible to the unaided eye; and in doing this you will notice that the conditions are entirely different from those commonly occurring in stellar photography. A photograph is ordinarily taken in the focus of a large telescope covering perhaps a region only 1 or 2 degrees in diameter, and in some cases photographing, by a sufficiently long exposure, stars down to the extreme limit of visibility. Photography has also been used largely for determining the positions of the stars and other

celestial objects ; but with regard to the determination of the light of the stars, less attention has been paid in that direction. The first observations that I know of, on that point, were made by Professor George P. Bond in 1858, which some of you may recollect were published in the 'Astronomische Nachrichten' in a paper on the stars ζ Ursæ Majoris and its companion. He made a determination of the relative brightness of those two stars by a measure of the diameters of their photographic images. Recently an important contribution in the same direction has been made by a member of this Society, Mr. Common, in his remarkable observation as to the probable variability of a star in the nebula of Orion, which appears to be shown by a comparison of the photographs he has obtained of that object on different nights. In all these cases you will notice, however, the problem has been with stars near together. Our problem was how to photograph the whole of the stars—how to photograph a large portion of the heavens on one plate, so that we could compare different stars often at a large distance apart. For that purpose we use a wholly different form from the ordinary equatoreal telescope. It is not unusual to construct photographic cameras so as take pictures of buildings subtending an angle of 60° or even 90° ; but in this case it is necessary to use a very small aperture, and therefore, in the case of a star, a very small amount of light. We find, however, that some of the best lenses with large angles will extend over as much as 20° (that is, 10° from the centre line), and that the distortion will be so slight as not to seriously interfere with the work ; at the same time that we are able to use large angular apertures. By procuring lenses of this kind with unusually large angular apertures and by using the most sensitive plate which has yet been prepared (and here I may say my brother has made a careful series of measurements of the sensitiveness of different kinds of plates, and which will shortly be published) the result was that we were able to obtain photographs of stars down to the fifth and sixth magnitude, and to photograph them in so short a time that it was not necessary to use clockwork ; the trail of the star as it passed over the surface of the plate was sufficient to give an image. This gives us a great advantage in several respects, as you will see from the method we employ in our work. The plate is only about 6 inches by 8 inches, and is divided into six equal portions ; and the camera is so arranged that a photograph can be taken first on one part, then on another of the plate, by moving the lens or the plate-holder, precisely as is done by photographers when they wish to take a number of photographs in quick succession. The mounting of the camera differs from that of an equatoreal in having a series of notches or stops, by which it may be moved instantly by exactly 15° , either in right ascension or declination, to an hour. Let us suppose we wish to begin our photography : we have one of the six portions of the sensitive plate under the lens. Since there is no clockwork attached, each star instead of producing a point,

produces a line of light, the length of the line being proportioned to the time of exposure. Wishing, however, to distinguish the stars one from the other, instead of having a simple line, we expose for 10 seconds, in which time the star is only able to make a dot. We then cover for 10 seconds, and expose again for 30 seconds, and the result is that we have a dot followed by a line on our plate; and this would be the case with all the stars in this region of 15° square above the fifth or fifth and a half magnitude. This entire operation occupies only a minute. In our first exposure we obtain a photograph of the region between 30° and 15° south declination and between $1\frac{1}{2}$ hour and half an hour west of the meridian. We then move the instrument one hour in right ascension, which we can do instantly by means of the catch I have just spoken of. The camera is then on the meridian. We expose again, and get images on the same part of the plate of all the stars in another portion of the sky. To distinguish these from the stars first photographed, we make our first exposure one of 30 seconds, then we have an interval of 10 seconds, then an exposure of 10 seconds, so that now we have a line and a dot. This operation takes less than a minute, leaving us still 10 seconds in which to make the change for the next exposure. In this third case only one exposure is made, viz. of 30 seconds, and with the camera one hour east of the meridian. Thus we have three classes of images—dots and lines, lines and dots, and lines alone. This completes the exposure for this portion of the plate, and we still have 30 seconds before it is necessary to take the next photograph, which will be taken on the second of the six divisions of the plate. We change the position of the camera now from the 30° to 15° south declination which we have had hitherto, and now we go from 15° S. to the equator. We repeat the three kinds of exposures in the three different positions of right ascension, and so on, repeating the operation over each of the six squares; and now we have covered the region from declination -30° to $+60^\circ$ and from $1\frac{1}{2}$ hour on one side of the meridian to $1\frac{1}{2}$ hour on the other. We have thus obtained in eighteen minutes, on a plate only 6 by 8 inches in size, a photograph of this entire region, which is about $\frac{1}{8}$ th of the whole heavens, omitting the region in the vicinity of the Pole, which will have to be discussed by another method. The work, so far, has been to a great extent preliminary; but I think we have sufficiently far advanced as to be able to see that there is probably no real difficulty in the matter.

A second portion of our work consists in the preparation of a photographic map of the whole heavens, and for that purpose we shall take fainter stars than those which will be photographed in the method which I have just described. I have here enlargements on paper of the original negatives. One of these shows a considerable portion of Orion. You will notice the three stars representing the Belt and the Sword-handle, and that, small as it is, the nebula is an interesting object. On a plate of this

kind we obtain stars as small as about the 8th magnitude, but they will not be as available for measurements as the brighter ones. But a photographic map of the heavens will have one important advantage. Supposing a new star is found, how are we to prove that it was not there before? If we have a series of photographs in any region taken over and over again under the same conditions, and we find no image of the supposed new star, and if then another is taken under the same conditions, and it is shown there, we may be very certain that a change has taken place. Otherwise it may always be urged that it is absent from our catalogues by mere accidental omission.

You will notice in the photograph of Orion that α Orionis is very faint. Now the reason for that is obvious. It is a red star, and its photographic effect is therefore comparatively slight. A promising method of measuring the colours of stars is thus suggested. Until we compare the rays of each different wave-length we cannot hope to correctly indicate the colours of the stars; meanwhile we may get a valuable approximation from the relative brightness as determined by the eye and by the effect on the photographic surface—in other words, when we have two equations of condition for determining relative brightness instead of one. I think we shall find that many stars will show the effect of colour in a very marked manner. I will mention one example where the appearance of a constellation is so altered by this effect of colour that its configuration in the photograph can scarcely be recognized by one only familiar with its appearance to the eye. In the constellation Cetus we have the three brightest stars, α , γ , and δ , arranged thus:—[indicating their respective positions on the blackboard]. Of these, α is distinguished as the brightest star, the other two being fainter. Close to α is a small star, scarcely noticeable to the eye unless the attention is called to it, and only visible then to really good sight. The magnitudes of α and of this star in its immediate vicinity are given as 2.7 and 6.3 respectively. On taking a photograph of that region, however, I was surprised to find that this small star was nearly as bright as α Ceti, and differed from it only by three tenths of a magnitude. On turning the telescope to it, it appeared to be of a deep blue, whilst α Ceti is of a reddish tint. With regard to the photograph of Orion which I have here, I would only say that, though it is an inferior enlargement, yet if I draw a circle on it 5° in diameter, I can count 180 stars within it on the paper print; on the plate there would be more. The effect of colour is also shown by a comparison of the number of stars in the photograph of Orion with that given in our photometric catalogue. I made a reduction of the stars obtained in the constellation of Orion in a region of from about 5° north declination to 5° south, and from 5 hours right ascension to 6 hours. On comparing the photograph with the catalogue I found about 16 stars that were common to the photograph and the catalogue; 16 stars which were in the catalogue but were too faint to be photographed;

and 5 stars which were in the photograph but not in the catalogue. In other words, a difference in the colour of the stars has brought out this striking difference. I may also add that the mean deviation shown by the measurement of two different plates was 0.21 of a magnitude, and in two measurements of the same plate 0.07 of a magnitude; so that by photography we shall have a precise means of comparing the light of stars under wholly different conditions than when they are observed by eye. (Applause.)

Mr. Christie. I am sure the Fellows present must be very grateful to Professor Pickering for the very clear explanation he has given of the work in which he has been engaged. We all know the splendid work he has done as regards eye-observations of the brightness of stars; and we must all be glad that he has now taken up another aspect of the question, *i.e.* investigating the differences in the light of the stars by means of photography, and that the work is being done so excellently. All here must have remarked the admirable manner in which Prof. Pickering has arranged his observations so as to give the greatest amount of information with the least expenditure of time and labour. (Hear, hear.) That is a very essential condition of success when the large amount of work that has to be done is borne in mind; because the question is not one of getting merely some observations of the brightness of stars, but of obtaining the utmost amount of information possible, with a limited expenditure of time and labour. There is one point Professor Pickering mentioned to which I should like to refer. He said that there are two coefficients concerned in the brightness of stars. I fully agree with him in that; but I would go a step further, and I would say there are three. In order to represent the colour of stars we want three factors—we want the amounts of three standard colours, which, when compounded, will give the brightness and colour of the star, or, what is equivalent to this, the amount of the pure colour corresponding in hue and the amount of white light which has to be mixed with it to match the light of the star. It is necessary therefore not only to have these eye-observations and photographic observations, but also to get something between the two, as it were. There is only one question I should like to ask Prof. Pickering, and that is the part of the spectrum to which his photographic plates are sensitive, which is an important point.

Prof. Pickering. In answer to that question, I have here a photograph of the spectrum as taken by these plates, which shows just the region over which they extend. The spectrum is seen perfectly from the F line to the H line, and the plate therefore is sensitive to rays of much greater wave-length than is the ordinary wet plate. I quite agree with the proposition that the light in each part of the spectrum must eventually be measured. You will perhaps allow me to describe a little device by which we are hoping eventually to effect this. Let us suppose we have a non-achromatic lens, producing a great difference in the position of the focus for the red and blue rays.

Now, if we take a photograph in the focus of the blue rays, the red light has little effect; and if the photograph is taken in the focus of the red rays, the blue light has little effect. If we now cover up the centre of the lens, not with an opaque substance but with a prism of very small angle, and vary the position of the plates, we may bring to a focus all the light of any desired wavelength and scatter the other rays so widely as to produce no effect. That is one of the devices we have now on trial; and if we do not succeed, I suppose we shall try something else. We may have a series of disappointments; but next time we meet I hope I may be able to show you success in some direction. (Applause.)

Mr. Common. I have listened with great interest to the remarks of Prof. Pickering. My chief feeling, I think, is that I should like a little more information. (Laughter.) I have worked at this subject myself, and have demonstrated that a photographic chart of the heavens has to be done, and can be done at any time. I have tried some two dozen lenses of various sorts, from half-inch aperture and about 5 inches focal length to 6-inch aperture and 2-feet focal length, with varying conditions, and I have been met with the difficulty that I see Prof. Pickering has experienced—the lenses will not cover nicely beyond about 10 degrees. I am having a lens made now under conditions which, I think, will enable me to get a plate of 15° or 20° that will show stars, with the proper exposure, down to the tenth magnitude with sufficient clearness to be capable of enlargement and of use as a chart. I can bear out what Prof. Pickering says with regard to the different way in which different stars, according to their colour, act on a photograph. I took a photograph of the constellation of Orion, including the brighter stars. I compared it with the charts, and I found stars not on the chart, and on the chart stars that were not on the photograph. I was so satisfied with the results of that plate, that all my future endeavours have been to get lenses that will cover more surface, before beginning a regular series of photographic charts with diameters of one hour or about 15° , till I get round the heavens. That, I think, will be most valuable; for a photograph has no prejudices and cannot make mistakes: what it sees it shows with fidelity. I should very much like some more data from Prof. Pickering, as to what lens, or what aperture or plate or exposures were employed. I should like to know, for example, how long that negative showing the three stars in the belt of Orion was exposed.

Prof. Pickering. One plate was exposed only half a minute; the other had an exposure of 30 minutes. Both the lenses were small. We find the smaller lenses the best when we do not use clockwork. The photograph showing the larger number of stars was taken with a lens of about 2 inches aperture and 7 inches focal length.

Mr. Common. That is a very large aperture for the length.

Prof. Pickering. It was a small lens.

Mr. Common. I find that with an aperture one third the focal length, an exposure of 20 minutes will give stars down to the eleventh or twelfth magnitude. With a lens properly made, I have no doubt I could get a field of from 15° to 20° in diameter; and that, I think, will be a great step towards a photographic chart of the heavens.

The President. I am sure you have already returned thanks to Prof. Pickering. We are sure they will do downright good work in America, and we will do better here if we can. (Laughter.)

The Rev. Father Perry. I have a short note on some observations made last week at my observatory at Stonyhurst. We do not hear sufficiently at present of what is being done in spectroscopy with regard to the Sun and the stars. We could direct our work to much better advantage if more were said as to what is being done in this direction; and I have therefore thought it would be worth while to bring under your notice an observation of the spectrum of the Sun made last week, while the ordinary observations of the chromosphere were in progress. I might mention that every fine day we examine the entire limb of the Sun, and measure the height of the chromosphere throughout its circumference, which can be done, unless there are a good many prominences, in a little over half an hour. A great deal has been said against the climate of England; but last year my assistant was able to construct 227 complete drawings of the Sun, showing the appearances of the spots and faculæ, which I am anxious to compare with the photographs of M. Janssen and others. I do not think that such details can be obtained by photographs as can be secured by the eye. The method we adopt is very simple, but effective. First of all, a very rapid outline-sketch is made of the spots and faculæ, the paper is moved a little, and then all the details are filled in. The drawing is then placed under the image of the Sun, so as to verify the position of the details as far as possible. The size of drawing I have adopted is $10\frac{1}{4}$ inches for the diameter of the Sun; but when large spots have appeared, I have taken these on a larger scale. But besides these observations of the spots, I also have the chromosphere observed on every day on which it is possible to do so. Last year we obtained 70 complete measures of the chromosphere, and we obtained incomplete measures on other days when there was a slight haze. The C line, which is the brightest of the lines of the chromosphere, is generally selected for this work. On May 30 we observed a new line between C and B. This new line is one well known from the observations of Prof. Young, and in his catalogue appears rather nearer C than B. The extraordinary part of it was that it nearly equalled the brilliancy of C, there being only a slight difference in intensity. This line is Kirchhoff's 654'3. A day or so afterwards we found that a large spot (not so large, however, as the one that has burst out since) was actually on the limb at the time of the observation of K 654'3. When we measured the chromosphere first, by means of the line C,

this line was not detected; but there was a prominence, I think about $63^{\circ} 58'$ to the east of the north point, where this line K 654.3 came into view. It was intensely bright at that point. At that time there was a curious formation about C. At the less refrangible side there was a little separate cloud. Clouds are very common in the chromosphere; but clouds displaced as that one was are not so common, at least we have not observed them often. The first time of going over this cloud the line K 654 was not noticed; but when my assistant returned to it, he saw the cloud come into view along with the line K 654. He went on with the measurement of the chromosphere and then returned to this place, as he thought something extraordinary was going on; the cloud had disappeared, whilst the line K 654 still remained, but not quite so bright as previously. When he came to the prominence the third time there was a double reversal of the line C—a bright red line and then a dark line (the ordinary Fraunhofer line) in the centre of it. I should like to know if this line K 654 is often seen in England. It seems to have been frequently seen by Respighi, according to a letter published in the 'Observatory' last year, which stated that it was seen fairly often in Italy; but it is doubted whether it is often seen in England. We have had some very fine days since, and have looked carefully for that line, but have not seen it. There is another line between B and α , but that was not in the field of view. I bring this observation before the Society specially, in order to get any little advice as to spectroscopic work on the Sun from the Fellows present, and also in order that I may compare results with those who may be willing to bring forward observations of this kind.

I should like also to call attention to this micrometer, which has been made by Mr. Hilger for a friend of mine in India (Mr. Nursing Row), as he had a difficulty in getting one there. It contains a double system of wires—one of platinum wires for solar work (it being very difficult to get spider-lines replaced in India), the other with ordinary spider-threads. But what I would draw special attention to is the method of lighting up the field. That has been the great difficulty with micrometers, and I do not think that hitherto it has been satisfactorily solved. The problem is to light up the whole of the wires in the field of view from one end to the other (right-ascension and declination wires together) at the same time. That is done here by means of a small vacuum-tube which is placed inside, on the object-glass side of the focus, and it is placed so as not to interfere in the least with the field of view. I observed with it the other day, and found it very satisfactory. The vertical wires are illuminated from one end to the other, and the wires at right angles to them also, so that there is a nearly dark field with the lines throughout the whole extent of the field, above and below, uniformly bright.

Mr. Common thought the difficulty was not quite settled with a vacuum-tube, unless it was a great improvement on one he tried

two years ago. There was always great difficulty with the batteries and the coils ; but the worst feature was the danger of getting a shock to the eye, unless the wires were perfectly insulated. When he tried an experiment, the first thing he experienced was the sensation that the illumination was very satisfactory ; and the second thing was a shock in the eye, which fairly settled that method of illumination for him. (Laughter.)

Mr. Mattieu Williams said the difficulty as to the renewal of spider-lines experienced in India, to which Father Perry had alluded, might justify him in making a few remarks how that difficulty might be overcome. He did not know whether many practical astronomers knew how to prepare their own collimator-wires or spider-lines ; but nothing was easier. In the first place, the right spiders must be caught and in the right season. They were easily found in this country ; he did not know whether they could get the geometric spider in India. When the spider was caught there was an easy way of applying the web to the micrometer. A piece of wire should be bent into the form of a fork ; the spider was then allowed to fall ; he spun a thread as he dropped, and the fork could be twisted round so as to take up the thread. A number of such wires could be prepared and stored ready for use. It was perfectly easy, by means of one of these forks, to lay the thread in the required position in the micrometer-frame, and a single drop of gum at each end would fix it in its place. He once met a surveyor in Italy who was in trouble from having lost the spider-thread of his theodolite, and he soon set him right, whereas otherwise he would have had to send his instrument to Milan for repair. The best means of fixing the thread was a solution of shellac in alcohol.

The President. Of course, in the colonies astronomers may be obliged to do their own work. The great difficulty with regard to the wires appears to be that the gum is apt to deteriorate in hot climates, and the wire will be lost ; and when it is replaced, there is a great deal of trouble required to determine the constants belonging to it.

General Boileau. I have had a good deal of experience in fixing spider-lines, and I always found the best solution was gum benzoin or friar's balsam ; it is thin, adheres sufficiently, and does not stick to the fingers. The description given by Mr. Williams as to the method of taking the webs from the spiders was given to me by Mr. Simms in 1839 before I went to India ; but his instructions were that the spider must first be put in a passion before you let him drop, and then you had every thing in your power, and all that was wanted was a good eye and steady hand.

Mr. Christie. We have wandered, I think, a little from the point that Father Perry brought before us. (Hear, hear.) I would like to remind the Fellows of the important results that may be obtained from observations of the Sun. As regards the spots, I

think there are two different classes of observations. There are photographs of the Sun taken with the object of determining the positions and areas of the spots, and drawings intended to give an accurate delineation of special details. Notwithstanding the improvements in photography, drawings, if carefully made, will show us more in regard to details than photographs will. I think, therefore, that it is very important that amateur observers should devote attention to the Sun when there are a great many spots, in order that important changes in their details may be observed and recorded. Many objects of interest may be seen by the casual observer. On the occasion referred to by Father Perry, the second and third examination with the spectroscope brought out very important results. But these results are not by any means singular. I think any observer who watches the Sun carefully and goes over his work several times will find results like these—displacement of lines in the spectrum, and clouds that appear suddenly and disappear with equal rapidity; and it is very important these things should be recorded. Of course it is necessary that a regular record should be kept of the positions and heights of the prominences by sweeping round once; but it cannot be expected that one observer should go on all through the day repeating that work. There is therefore an opening for amateur observers to take up an occasional examination of the Sun with the spectroscope. I may mention that, as far as I recollect, this line to which Father Perry calls attention has been observed at Greenwich once or twice.

The President said that he thought eye-observations had been too often disparaged and doubt cast upon them; nevertheless it was a great advantage to be able to appeal to photographs.

Mr. Hilger explained the method of illumination employed in the micrometer exhibited by Father Perry. He had placed the vacuum-tube all round below the webs, but not touching them, and thus had obtained perfect illumination; and if any accident happened to the vacuum-tube it could be taken out easily.

The Rev. Father Perry said, in answer to *Mr. Common*, that he had observed with this vacuum-tube, and had not received any shock such as *Mr. Common* had spoken of.

Mr. Hilger said he had taken especial care that the vacuum-tube should be well insulated; and there was not the least danger of disagreeable shocks, as the terminals were placed as far off as possible.

Mr. Newall said there was something worse than shocks, and that was the high temperature of the vacuum-tube, which he found to interfere with the work.

Mr. Hilger replied that he did not find that the tube got warm, as the heat was only at the terminals; and as the tube was $4\frac{1}{2}$ inches long, there was no sensible heat in the centre.

The President understood that in this case there was circular illumination, which undoubtedly was the proper form.

Mr. Christie asked why the vacuum-tubes were placed behind the wires on the side further from the eye.

Mr. Hilger said it was for an obvious reason—because there was no room in front. Some persons had tried to illuminate from the front, but that would only do for very low magnifying-powers.

Mr. Christie. Space might be gained by bringing the lenses of the eyepiece together.

Mr. Hilger. That is the French system, which seriously interferes with the flatness of the field.

Mr. Christie said the illumination would be very much improved; but it is a question of balance of advantages and disadvantages. With the illumination coming from the two sides, the light will be scattered, and the wires will appear broad and darkest in the middle. If we could have very faint lines it would be an improvement; and by throwing the light on the wires from the front that might be obtained.

Prof. Pritchard asked whether *Mr. Hilger* could undertake to illuminate twenty lines engraved on a plate of glass at intervals of a tenth of an inch apart, and arranged in two sets at right angles to each other, ten lines being in each set. If he could do so it would be a great advance.

Mr. Hilger said it would require some consideration; but he had no doubt the problem could be solved, and would not fear to pledge himself to its solution.

The President said that it was only fair that *Mr. Hilger* should have time to consider the question, and perhaps he would prepare a paper on the subject.

Mr. Knobel read a note by *Prof. Adams* on Delaunay's expression for the Moon's parallax.

The President. I may take the opportunity of saying that the investigation of *Prof. Adams* on the parallax of the Moon many years ago was a step of extreme importance to practical astronomy. At the time when we referred to *Burckhardt's* parallax for corrections of the semidiameter of the Moon, matters had got into great confusion, till *Prof. Adams* took up the question, and was able to determine the Moon's semidiameter with some close approximation to the truth. It is curious to see how closely *Prof. Adams's* value agrees with the value produced by *Hansen* and *Delaunay*.

Mr. Knobel read a paper by *Dr. Gill* on a Telegraphic Determination of the Longitude of the Cape Observatory, which he found to be $1^{\text{h}} 13^{\text{m}} 54^{\text{s}}.742$ east of Greenwich.

The President. The longitude of the Cape Observatory now determined by *Dr. Gill* is within a quarter of a second of that determined by *Henderson* many years ago. I may mention that, when I was at the Cape, I worked up the point from 1856, and got a correction of -1 sec., and for 1857–8–9 I got smaller corrections again; and as they agreed so closely with *Henderson's* result, I thought it would be useless to go further. It is satisfactory to

find that the result of the lunar observations has not been altered by this determination of Dr. Gill's more than a quarter of a second.

Before the Meeting closes, you will excuse me if I refer to a communication of my old friend, Sir George Airy, in the 'Observatory,' expressing his dissent from the views I have brought forward with regard to a change in the unit of time which, I believe, had taken place since the adoption of Hansen's Tables in 1864. It is a question of considerable difficulty; and all I can say is, that having looked over the entire matter again with great care, I do most respectfully but firmly adhere to the views I have already expressed.

The President then briefly explained his views on the question at issue.

The following papers were also announced:—

A. Brown. "The Annular Solar Eclipse of 1836, May 15."

W. H. Finlay. "Determinations of Longitude on the East Coast of Africa."

J. Gledhill. "Phenomena of Jupiter's Satellites, observed at Mr. E. Crossley's Observatory, Bermerside, Halifax, during the Opposition of 1882-83."

Rev. T. E. Espin. "Observations of U Monocerotis and LL 14551 with a new photometer."

Communicated by *E. J. Stone.* "Observations of Occultations of Stars by the Moon and of the Phenomena of Jupiter's Satellites, made at the Radcliffe Observatory, Oxford, 1877-78 and 1882-83."

J. Tatlock, Jun. "Elliptic Elements of Comet *b* 1882."

A. Marth. "Ephemeris of the Satellite of Neptune, 1883-84."

A. Marth. "Ephemerides of the Satellites of Saturn, 1883-84."

A. Marth. "Ephemeris for Physical Observations of Jupiter, 1883-84."

A. Marth. "Postscript to Note on M. Loewy's communication referring to an apparatus for the determination of flexures."

The following gentlemen were duly elected Fellows of the Society:—Lieut. W. H. Coombs, R.N., T. K. Mellor, John Pender, M.P., and W. J. V. Vandenberg.

The Meeting adjourned at 10^h o'clock.

Dimensions of Saturn from Observations with the 10-inch Refractor of the Observatory at Geneva, made during the Opposition of 1881.*

THE micrometer measures of Saturn and its satellites which I have executed in the year 1881 were obtained in an essentially different manner from those of 1880. In my first application of

* *Astronomische Nachrichten*, No. 2517. (Translation.)

the great instrument to such delicate observations, it was inevitable that I should not at once hit upon the best arrangement of the observations. At that time I always put the fixed wire upon the extremity of the ring-system, and then turned the movable wires successively upon all points of the system the distances of which with respect to each other I wished to measure. In this manner, for example, the equatoreal diameter of Saturn was determined by every two positions of the movable wires, both of which were at some distance from the fixed wire, kept always as a tangent for the ring-system. By this procedure the dimensions of the system on the east and on the west were determined with two different parts of the micrometer-screw, a course which always introduces an uncertainty into the observations, when the question is about such trifling differences between two symmetrically placed distances as those from which an excentric position of the ring with respect to the ball, supposing it to exist, could be proved. During the opposition of 1881, I have measured the most interesting dimensions of the system on 22 evenings in such a manner that every dimension was the result of an independent series of observations. I placed, for example, first the fixed wire upon the one extremity of the system, and the movable wire upon the other, repeating this five times in the one and five times in the other position of the micrometer. Then a similar series was made for the interior diameter of the ring, for the equatoreal diameter of Saturn, &c. In order to decide, so far as my means permitted, the question of the excentric position of the ball within the rings, I have measured the distance of the extremity of the ring from the nearest limb of the planet on the right and on the left separately, so that precisely the same parts of the instrument were used in the two separately obtained results. It will be seen from the annexed Table that it is not possible to perceive any excentric position of the ring from the observations of 1881.

The 22 evenings referred to fall between the the 16th of September and the 9th of December. Measures were only made during comparatively favourable atmospheric conditions. The dimensions quoted in the column "1880" are taken from those given in the '*Recherches sur Saturne*,' differing, however, by some hundredths of a second, on account of the improved correction for the micrometer-screw since that time. The column "Diff." gives the difference 1881 - 1880; it shows a small preponderance of the negative sign. The column headed $\pm$ exhibits the mean error of the results determined in 1881, deduced from the agreement of the separate evenings; lastly, the column "Number" gives the number of evenings of observation for the corresponding distance. On each evening, 10 readings were taken for each distance, so that the above series of observations includes 1430 readings.

The interior diameter of the bright ring could not be measured with very great accuracy, since the interior edge of the ring did not appear sharply bounded. That the mean error of a measure

Dimensions of Saturn for the distance 9'5389.

	1880.	1881.	Diff.	$\pm$	Number.
Exterior diameter of the bright ring.....	40'43	40'35	-0'08	0'038	22
Diameter of the ring in the middle of Cassini's division ..	34'48	..	..	0'098	4
Interior diameter of the bright ring.....	26'30	26'05	-0'25	0'059	21
Interior diameter of the dusky ring.....	21'15	21'13	-0'02	0'085	5
Distance between the extremity of the ring and the planet on the west ..	11'59	11'34	-0'25	0'026	20
The same distance on the east	11'41	11'30	-0'11	0'038	20
Equatoreal diameter of the planet	17'40	17'77	+0'37	0'028	21
Polar diameter of the planet	16'18	16'12	-0'06	0'055	20
Compression	$\frac{1}{145}$	$\frac{1}{107}$	..	..	..
Minor axis of the ring on October 26th.....	..	14'19	..	0'065	10

of this dimension is much greater than that for the other dimensions must be ascribed to this circumstance.

The agreement of the observations of the dusky ring in 1881 with those in 1880 is remarkable, but is to be considered as only casual, since in 1880 this dimension was only measured on two evenings. The separate results of the five evenings in 1881 agree together very well; the mean error of an evening is $\pm 0''\cdot 19$.

The small correction due for the phase of the planet has been applied to each mean for the distance of the planet's limb from the extremity of the ring.

The equatoreal diameter of the planet was found to be perceptibly greater in 1881 than in 1880. But to the result last obtained a considerably greater weight is due, as it has been directly obtained; and besides the compression of Saturn deduced from comparing it with the polar diameter agrees well with that obtained by the majority of other observers, whereas the compression determined from my observations in 1880 is markedly small in comparison with that found from other series of observations.

The polar diameter was directly measured both in 1880 and in 1881, on which account also the difference between the two series is not considerable. The correction for the diameter as measured

upon the plane of projection at the time to that of the pole has been applied. It amounts for 1881 to $0''.177$ in the mean.

The deviations of these values given above for the diameters from the values communicated a short time since, which were deduced from the observations of the satellites in 1881, are respectively $-0''.09$ and $+0''.25$.

The minor axis of the ring could only be observed subject to some degree of uncertainty, as the part of the ring nearest to us was projected upon the planet's disk, and the limb of the ring in this direction could only be recognized by the difference of the shading. On the southern side the limb was covered by the planet's disk, and the observation was taken at the points where the peripheries of the ring and of the planet intersected each other, a correction being applied to obtain the length of the minor axis from the observed distance. It is of course to be ascribed to this unfavourable position of the ring that these observations, repeated on ten evenings and agreeing well with each other, when reduced to the plane of projection of the 26th of October, give a considerably smaller value than that assigned in the '*Berliner Jahrbuch*.' The difference amounts to $-0''.68$. The value in the '*Berliner Jahrbuch*' is derived from Bessel's dimensions, which, on the other hand, give the major axis of the ring smaller than my observations do. A yet smaller value of the projected axis results from the observations in 1880.

On the 7th of November I measured the distance of the dark belt upon the ball of Saturn from the southern limb of the planet, and find $9''.21$ for the northern edge of this ball and $4''.72$ for the southern edge. From this it results that this belt extends from -16° to -47° of saturnocentric latitude. I also noted that "the upper (southern) edge of the belt is not quite so sharply defined as the lower, and the polar cap which commences upon it is decidedly darker than the equatorial region, but brighter than that belt."

With regard to the difference of colour between the ring and the ball, I recorded the following short note on November 4th:—"I have remarked for some time past a very conspicuous difference between the colour of the light of the ball and of the ring. The ball is darker, paler, and of a greyish-blue tint, whilst the ring appears to me of a glittering white colour. The dark belt upon the ball is very sharply defined. Encke's division and the dusky ring appear to-day of a remarkable distinctness."

As to the excentric position of Encke's division, which I perceived after it had been pointed out by Prof. Schiaparelli, I will conclude this with my original notices, referring at the same time to the article by that astronomer in '*Astr. Nach.*' 2430, "*Sopra alcune osservazioni fatte in Saturno ed in Marte*"*.

"Nov. 30. I sometimes see Encke's division distinctly. I consider that it is situated on the left nearer to the exterior edge, so

* Translated in the '*Observatory*,' Vol. v. pp. 221-4.

that it divides the exterior ring in the proportion of one to two, whilst on the other hand its position on the right side is at almost an equal distance from the outer edge and from Cassini's division.

"Dec. 3. Power 350; image 2-3, at times very steady. I see distinctly that Encke's division is placed nearer to the exterior edge on the left than on the right ansa; on the latter side, it is nearly in the middle of the exterior ring, perhaps even a little more towards the interior; but on the left side it seems to divide that ring into one third and two thirds. I write this without having taken knowledge of the remark on the preceding page or even remembering it clearly.

"Dec. 6. I see Encke's division, as before, excentrically placed. Herr Kammermann, the second assistant, who has great difficulty generally in seeing the division, at last thinks that he perceives it on the left nearer to the outside, thus agreeing with me."

DR. M. WILHELM MEYER.

The Progress of the Numerical Lunar Theory.*

THE investigation of the Numerical Lunar Theory, conducted principally at public expense, was commenced in consequence of a recommendation to that effect, originating within the Board of Visitors itself, and addressed by the Board to Her Majesty's Government, at their Visitation-Meeting of 1874, June 6. The calculations, which are very heavy, have advanced slowly; but this will cause no surprise to those who have witnessed the constantly increasing occupations of the Royal Observatory, and the pressure produced by the Transit of Venus, 1874. As the work has now reached a stage at which important information on its primary results can be given, I think it my duty, without further delay, to report to the Board the steps that have been gained, and the course which I am now following.

After forming the Three Fundamental Mechanical Equations of the Theory—called Equations (10), (11), (12), and applying respectively to the directions "Transversal to the Ecliptic Radius Vector," "Ecliptic Radius Vector," and "Normal to the Ecliptic Plane,"—

Delaunay's expressions for $\frac{\text{Moon's mean distance}}{\text{Moon's true distance}}$ (as inferred from

his Parallax), Moon's longitude, and Moon's latitude, all referred in respect of longitudinal measure to the same unit ($10^{-7} \times$ that radius vector a which corresponds to mean parallax) have been substituted in the three Equations, which they ought to satisfy, not only in the gross but also for every separate Argument. The result of the substitution is that for the Equation Normal

* Extract from a Memorandum addressed to the Board of Visitors of the Royal Observatory, Greenwich.

to the plane of the Ecliptic the agreement is good, but for the other Equations the discordances are intolerably large. No variation of the numerical measures tacitly assumed (as the Moon's mass, the motions of node or apse, &c.) tends to diminish them. It is impossible to avoid the conclusion that there is great error, either in my calculations or in Delaunay's.

And now arises the consideration of the treatment of these discordances. The first presumption is that the error is on my side. But every part of my calculations has been made independently in duplicate, and most of them in triplicate; and they have been compared by Assistants in whose accuracy I could trust. I shrink, therefore, from the labour of re-examining this very large work, not only as great, but also as unnecessary. At the same time, I have no right to attack M. Delaunay's accuracy without due exhibition of an error or of a source of error, or further proof of my own accuracy. Under these circumstances I have adopted a middle course. The whole number of my Arguments and consequent comparisons exceeds 200 in each ordinate. I have limited my attention to 15 in each of the ordinates; for these I am at this time re-computing, or rather checking, the two Fundamental Equations (10) and (11), by a process which does not make use of the same figures as in the original calculations. For instance, the serieses for $\left(\frac{a}{r}\right)^2$ and $\left(\frac{r}{a}\right)^2$ are computed from certain expressions formed by use of the binomial theorem, and are to a great extent independent. But $\left(\frac{a}{r}\right)^2 \times \left(\frac{r}{a}\right)^2 = 1$, and therefore the product of their two serieses ought to = 1. Upon performing this multiplication, with absolute completeness in all possible combinations for every one of the 15 terms but no further, the discordance is found to be extremely small. This appears to offer a strong presumption that there has been no computer's blunder in these calculations.

Anticipating the probability of discordances of moderate amount, I had arranged from the first to prepare the differential coefficients for each of the Three Fundamental Equations, applying to every sub-coefficient for each Argument. The formulæ for the calculation of these coefficients are not very complicated. I have now computed their numerical values, and have applied them as factors of the yet undetermined corrections to each sub-coefficient in the series which represents in its totality the Grand Coefficient for each Argument. This is done separately in each Fundamental Equation. As is mentioned above, I have limited the number of Arguments to 15 (those whose parallax-coefficients are largest), and have limited the numerical value of the coefficients, in descending magnitude, to 0.01.

I have drawn up the equations formed by comparing the Delaunay-Airy discordances with the factors of corrections of Delaunay-coefficients that appear necessary to remove the discordances. The following remarks will explain the notation of the equations.

δM is the correction-factor of terms depending on the Moon's mass; δF and δL those for terms depending on the motions of node and apse.

g_1, g_2, g_3, g_4 , &c. are the numerical sub-coefficients of the terms $\cos. O, \cos. l, \cos. |2D-l|, \cos. |2D|$, &c., in the expression for Parallax; and $\delta g_1, \delta g_2, \delta g_3, \delta g_4$, &c. are their symbolical corrections.

h_2, h_3, h_4 , &c. are the numerical sub-coefficients of $\sin. l, \sin. |2D-l|, \sin. |2D|$, &c., in the expression for Longitude; and $\delta h_2, \delta h_3, \delta h_4$, &c. are their symbolical corrections.

$k_{301}, k_{302}, k_{303}, k_{304}$, &c. are the numerical sub-coefficients of $\sin. f, \sin. |f+l|, \sin. |f-l|, \sin. |2D-f|$, &c. in the expression for Latitude; and $\delta k_{301}, \delta k_{302}, \delta k_{303}, \delta k_{304}$, &c. are their symbolical corrections.

The number of equations is 44, and the number of unknown quantities is the same. I have not yet entered actively on their solution.

The work up to this point is drawn up in orderly arrangement, and might at once be placed in a compositor's hands.

G. B. ATRY.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Saturn's Ring.

SIR,—

The letter of Mr. W. T. Lynn on "Saturn's Ring" ('Observatory,' June) induced me to see whether I could find (in the manuscripts of Christiaan Huygens in the library of the Leiden University) an answer of Huygens to Sir R. Moray's request for observations of Saturn in 1666. There are a great number of letters from Moray to Huygens, but very few copies of letters from Huygens to Moray. If Huygens's letters still exist, they are probably in England.

The letter of Sir R. Moray to Huygens, to which an allusion is made in the paper in the 'Philosophical Transactions,' is in the library at Leiden. The letter is dated, "Alberry proche d'Oxford ce 8 Jan. 1666." After some remarks on the springs in the clocks after Hooke's construction, and on an observation of the transit of one of Jupiter's satellites over the planet's disk, he continues:—

"Quant au plus éloigné (la planète Saturne) il y aurait plaisir à découvrir que son corps tourna autour de son axe pendant que ses anses demeurent fermes, et si c'est un cercle comme vous avez prouvé qu'il *pourra bien estre* sans difficulté il ne pourra avoir aucun mouvement autour du corps du planète si ce n'est qu'il garde toujours une même plaine. Mais il est temps que vous consideriez avec grande attention la figure de ces anses à présent pour voir

si vous n'y voyez rien qui vous fasse avouer que ce n'est pas un corps de figure circulaire qui embrasse la disque du planète, mais deux ; ce qu'il y aura quelque (lieu ?) de croire à ce qu'il en semble selon les observations qu'en ont fait le Sr. Ball et son frère tous deux de nostre société avec un fort bon télescope de 36 pieds, dont Mr. Oldenbourg vous enverra aussi tout le détail ; selon qu'il me l'a envoyé il y a quelques mois et que je vous aurais fait tenir il y a longtemps si je n'eusse cru que n'estiez plus en Hollande ayant appris que le Roy de France vous avait convié d'aller demeurer à Paris pour y établir une Academie pour la philosophie naturelle, comme la société Royale, ce qui m'a fait aussi tarder si longtemps à vous escrire en attendant de vos nouvelles.

"Il est maintenant hors des rayons du soleil ou il entraît lorsque je priai Mr. Ball de repeter avec soin ses observations et il ne m'en dit rien depuis, mais je prétends l'y convier encore quoiqu'il ait le corps assez valetudinaire."

Whether the word "lieu," which I put between brackets, is the right one, I cannot positively say ; it was very difficult to decipher it.

From this letter it seems that Moray is not convinced by Huygens's paper on Saturn's ring ; the words "qu'il pourra bien estre" in the beginning of the letter are underlined by Moray himself. Moray thinks it possible that there are two rings, as is indicated in the figure of the 'Phil. Trans.,' of which a copy is given by Prof. Adams in the 'Monthly Notices' of the R. A. S.

I do not know whether Huygens made any observations of Saturn at the request of Moray or not ; but in a book, wherein Huygens has written the greater part of his astronomical observations, and which is also in the University's library, I find no observation of Saturn between 1665 April 14 and 1668 July 14 ; probably he was too much occupied with other work in Paris, whither he went in the spring of 1666.

It is clear that Ball is not the discoverer of the division of Saturn's ring, but that Cassini ought to be accounted the discoverer is not quite so certain. In the above-mentioned book, with manuscript observations of Huygens, there is a drawing of Saturn made 1675, Dec. 8, which has been copied by Prof. Kaiser, and published in a paper, "De stelling van Otto Struve omtrent het breeder worden van den ring van Saturnus getoetst aan de handschriften van Huygens, en de nauwkeurigheid der latere waarnemingen," in the third volume of the 'Verslagen en mededeelingen der Kon. Akademie van Wetenschappen,' Amsterdam, 1855. The division of the ring and the difference of brightness of the two parts is there clearly indicated. Above and on the side of the drawing Huygens wrote—"8 Dec. 1675 hora 5 post merid. Saturnus cum comite observatus tubo 36 pedum Campani ; aderat de Cassinius. Ad A. umbra globi in annulum projecta, manifesto apparebat. Planum annuli non æque lucidum undique videbatur, sed parte dimidia exteriori obscurius erat quam reliqua, et utriusque confi-

nium distincte terminatum circulo *bb*; quod a Josepho Campano jam olim observatum, ut figura ab ipso edita comprobatur. Apparebat etiam ductus obscurior paulo *cc* qua parte annulus ante globum obijcitur. Item in medio globo fascia *dd*, sed vix conspicuenda."

When Huygens made this observation Cassini was with him; but from the notice in the 'Phil. Trans.,' it is probable that Cassini saw the division of the ring in August or September 1675; so that there is no sufficient ground to think that it was Huygens who showed the division to Cassini.

But there is still another astronomer named by Huygens: he says that "Joseph Campani has already observed the two parts of the ring, as is proved by a figure which he has published." I searched after that figure in vain in different books till I found, between a number of letters addressed to Huygens from Leopold, Prince of Etruria (the same to whom Huygens dedicated his 'Systema Saturnium'), a sheet of paper with two printed drawings of Saturn and Jupiter, and the following printed inscriptions:—At the head of the sheet: "Eminentissimo principi Flavio Chisio S. R. E. Cardinali. Quod in Saturno et Jove vitreis lentibus torno a se nuper invento formatis deprehendit et Romæ primus oculis spectandum exhibuit Josephus Campanus. Obsequii et observantiæ argumentum. D. D." At the bottom: Romæ Nonis Julii 1664 delineatæ phases sunt inversa situ. Julii die 30 h. 2½ noctis latorem Jovis fasciam obscuram perambulabant maculæ duæ obscuriores, quas Celeberrimus Astronomus Cassinus Authori primum indigitavit; easque umbras satellittum dixit Jovem subeuntium, qui deinde ab ejus occiduo margine vero emergere visi sunt. Superiorum permissu."

The diameter of the globe of Saturn is 68·5 millimetres and of Jupiter 78·5. On the disk of Jupiter are three broad and one smaller belt, with the shadows of two satellites on one of them. there are some details in the belts which show that Campani's telescope was a very good one; the diminution of light towards the limb is also very conspicuous.

The shadow of the ring is to be seen on the disk of Saturn, and the outer part of the ring, for somewhat less than half the total breadth, is dotted, whilst the inner part is bright. There is no line between the two parts; but they are distinctly separated from one another by the difference in brightness. One can also see traces of the inner dark ring.

It is highly probable that the words of Huygens, "quod a Josepho Campano jam olim observatum, ut figura ab ipso edita comprobatur," refer to this figure of Saturn, which Huygens had probably received from Prince Leopold.

I can therefore conclude that Joseph Campani was the first astronomer who, by means of a very good telescope made by himself, saw distinctly the darker and the brighter part of the ring in 1664. It is, however, possible that Cassini was the first who saw the line of separation.

As it seems that Campani's drawings are not generally known among astronomers (they are not in the catalogue of the Pulkowa library), I will take a photograph of them and send it to you.

In the copy of the 'Philosophical Transactions' which belongs to the Academy of Sciences in Amsterdam, the plate with Ball's figure is wanting.

H. G. VAN DE SANDE BAKHUYZEN.

Leiden Observatory,
1883, June 10.

P.S.—I have found this morning, June 28, that the drawings of Saturn and Jupiter made by Campani are printed in 'Stanislaw Lubieniec de Lubienietz Theatrum Cometicum,' Pars prior, p. 574. Lubienietz received the drawings from Athanasius Kircher in Rome.

NOTES.

ROYAL OBSERVATORY, GREENWICH.—The Astronomer Royal, Mr. W. H. M. Christie, presented his Report for the twelve months ending 1883, May 20, to the Board of Visitors at the Annual Visitation of the Royal Observatory on June 2. In the first section relating to the Buildings and Grounds, the most interesting statement is that referring to the presentation of the Lassell two-foot reflecting telescope by the Misses Lassell, which has necessitated some alterations in the buildings and grounds. It has been mounted in the South Ground, and a circular building 30 feet in diameter has been erected in preparation for the dome. The dip and deflection instruments have in consequence been moved from Magnetic Office No 7 to the New Library. Under the head of instruments it is stated that the instruments purchased for the Transit of Venus 1874, after being used for the observation of the Transit of Venus 1882, have for the most part been returned to the Observatory. Two of the photoheliograph mountings (to which 6-inch object-glasses have been applied) and a photoheliograph tube have been lent to the Eclipse Expedition 1883; a transit and an altazimuth have not yet been returned after observation of the Transit of Venus in Brisbane. The "Naylor" equatoreal, an altazimuth, and a clock (Dent 2015) were lost in the wreck of the 'City of Brussels,' on the voyage back from Bermuda. A photoheliograph tube is at South Kensington, and another is in India. Two 4-inch telescopes were lent to Captain Wharton, R.N., for observation of the Transit of Venus in the Straits of Magellan, and have not yet been returned.

The re-arrangement of books consequent on the building of the New Library has been completed. A number of complete sets of the Greenwich publications have also been transferred to this building from the Dépôt to diminish the risk of the total destruction of the stock by fire. A number of duplicates have, at Dr.

Doberck's desire, been presented to the new Hong Kong Observatory.

The new mountings of the collimators of the Transit-circle, which were under construction at the date of the previous Report, were brought into use last summer, and have been found to work well in practice. They allow of the collimators being turned on one side about centres below when not in use, and thus it has been found practicable to observe stars by reflection as far as Z.D. $71\frac{1}{2}^\circ$, an alteration having also been made in the mounting of the mercury trough by which it has been raised about a foot. The stability of the collimators during each determination of collimation is regularly tested by observations for coincidence of the corresponding wires of the two collimators, taken immediately after as well as before the observation of the collimators with the transit-circle, and is found to be perfectly satisfactory.

The regular subjects of observation continue be the Sun, Moon, planets, and fundamental stars, with other stars from a selected list. The working catalogue of 2,500 stars down to the fifth magnitude having been cleared off, a new working list of 2,600 stars, comprising all stars down to the sixth magnitude inclusive, which had not been observed since 1860, has been prepared, and was brought into use at the beginning of March. About 1,200 stars were observed in 1882, the total number of observations with the transit-circle made in the year ending 1883, May 20, being 4488 transits and 4485 zenith-distances. Comet *a* 1882 has been observed seven times on the meridian since the date of last Report, and Comet *b* 1882 has been observed three times. From the beginning of this year, a correction of $-0''.39$ has been applied to the results of the Nadir observation to make them agree in the mean with the results of reflection observations of stars. Determinations of flexure were made on 1882, December 30, and 1883, May 13 and 18, the resulting values being $-0''.07$ and $-0''.78$ and $-0''.30$; but the observations on May 18 were not altogether satisfactory. No correction for flexure, as apart from the correction for R—D, has been applied to the observations.

The correction for R—D, the error of assumed colatitude, and the position of the ecliptic have been investigated for 1882. For the planetary results, errors of R.A. and N.P.D. have been formed, but the heliocentric errors have not yet been computed. The reflection observations of stars available for investigation of the R—D discordance extend from Z.D. $71\frac{1}{2}^\circ$ north to Z.D. $70\frac{1}{2}^\circ$ south, and the discussion of these shows discordances steadily increasing from the zenith towards the horizon, and amounting to $-1''.58$ for the group at Z.D. $68\frac{1}{4}^\circ$ north and to $+1''.66$ at Z.D. 70° south, a correction of $+0''.16 \sin Z.D.$ having been first applied to the reflection observations for inclination of the vertical at the mercury trough. It is quite evident that the discordances do not follow any such law, as $a + b \sin z \cdot \cos^2 z$, which was used from 1862 to 1880. Assuming the law $a + b \sin z$, which was adopted in the years preceding 1862 and in 1881, the R—D correction for

1882 would be $+0''.07 + 0''.42 \sin z$, and for the sake of continuity in the system of reductions this correction has been provisionally adopted for use in 1882. But the discordances between this formula and the observed quantities increase regularly from the zenith towards the horizon, amounting to half of the observed quantities at Z.D. 50° to 60° . The formula $+0''.08 + 0''.29 \tan z$ represents the observations better, though even this does not give sufficiently large results at large zenith-distances. In this discussion corresponding reflection and direct observations made on the same day have alone been used.

In order to adapt the altazimuth to the occasional observation of bright comets, a new system of wires, having central cross wires thicker than the others, was inserted at the beginning of this year. Since the lunation ending 1882, July 9, the observations of the Moon with this instrument have been restricted to the first and last quarters of each lunation, it being considered that the intermediate semi-lunation is sufficiently provided for by the meridian observations. It is proposed, however, to make occasionally determinations of the Moon's diameter in azimuth and zenith distance at the time of Full Moon. 317 azimuths of Moon and stars and 176 zenith-distances of the Moon have been obtained. Azimuths and zenith-distances of Comet *b* 1882 were observed on a single day. The Moon's diameter has been measured—

With the transit-circle, twice in R.A., 17 times in N.P.D.

With the altazimuth, 4 times in azimuth, 10 times in Z.D.

The most noteworthy addition to the *matériel* of the Observatory is the Lassell two-feet reflecting equatoreal, which has been generously presented by the Misses Lassell. With respect to this noble addition to the instruments under his control, the Astronomer Royal says, "The exceptional qualities of this fine telescope (with which Hyperion was discovered in 1848) are well known, and there could be no hesitation in accepting on the part of the Admiralty the offer of such a valuable gift. The instrument was removed from Maidenhead early in March, and has been erected in the South Ground, where it commands a nearly unobstructed view of the sky to within about 5° of the horizon. A circular building 30 feet in diameter has been erected for the Lassell telescope, and the construction of a suitable dome is authorized. There are two large mirrors available for use, and I contemplate taking advantage of the firm mounting and perfect clock movement of the South-east equatoreal to mount the spare mirror on this instrument, attaching it to the tube of the refractor, so as to have on the same mounting a refractor and reflector with their axes parallel. The former would be available for eye observation, whilst the latter could be used on the same object for physical work, spectroscopic or photographic. The Lassell telescope itself would be well suited for observation of faint satellites and comets which are beyond our present instrumental means."

With the various equatoreals, or with the altazimuth, 11 occul-

tations of stars by the Moon (7 disappearances and 4 reappearances) and 37 phenomena of Jupiter's satellites have been observed. The great Comet *b* 1882 was observed on ten nights, Comet *a* 1883 (Brooks-Swift) on 4, and Comet *c* 1882 (Barnard) on one night. Micrometer observations of six of Saturn's satellites (including 2 measures of Enceladus) were made with the South-east equatoreal on 9 nights. Four measures of Titania, the third satellite of Uranus, were also obtained on one night. A number of observations of the double stars Castor, γ Leonis, γ Virginis, and ζ Ursæ Majoris have been made on 6 nights with the Airy double-image micrometer on the Sheepshanks Equatoreal, mainly with a view of practising the observers in micrometer observations.

Preparations were made for observing the Transit of Venus on December 6, but clouds completely hid the Sun from view during the time of the transit.

With regard to the spectroscopic observations it is stated that they have been somewhat restricted through the pressure of the photographic reductions at a time of maximum of sun-spot frequency. The solar prominences have been observed with the half-prism spectroscope on 8 days, and four sun-spots have been examined on 8 days with reference to the broadening of lines in their spectra. The spectrum of the great spot of 1882 November 12-25 showed some remarkable reversals of the lines of hydrogen and sodium, and an extraordinary displacement of the F line. As regards the determination of motions of stars in the line of sight, 142 measures have been made of the displacement of the F line in the spectra of 23 stars, and 26 measures of the b_1 line in 9 stars. The observations of Sirius during the past winter tend on the whole to confirm the impression that the rate of recession of this star has diminished progressively since 1877, and that the motion is now on the point of being converted into one of approach. The spectrum of Comet *a* 1882 was examined on three nights, that of the great Comet *b* 1882 on three nights, and that of Comet *a* 1883 on one night. The spectrum of the first-named object showed the yellow sodium lines with great brilliancy just before perihelion passage. The spectrum of the aurora of 1882 November 17 was also examined.

Photographs of the Sun have been taken on 200 days, and of these 339 have been selected for preservation. There were 7 days on which the Sun's disk was observed to be free from spots. The number and size of spots and faculae continued to increase in a marked way till last November, when a group of spots of very unusual size appeared. Since that date, however, the Sun has become more quiescent. Since the beginning of December gelatine dry-plates have been used instead of the old wet-plate process. They are more convenient in use, and appear to give as good average results. The photographs on a scale of eight inches to the Sun's diameter recently obtained in India, under the auspices of the Solar Physics Committee, are so successful that the Committee

have recommended the general adoption of this scale; and the Astronomer Royal proposes so soon as the spare photoheliograph is returned from the Eclipse Expedition, to have it altered in the same manner as the Indian photoheliograph, so as to obtain eight-inch photographs of the Sun instead of four-inch. It was suggested in the last Report that the measurement of such of the Indian and other photographs as were required to fill up gaps in the Greenwich series might with advantage be undertaken at the Royal Observatory. This proposal has now been carried out, and 111 photographs for the period from 1881 December 22 to 1882 October 19 have been received from the Solar Physics Committee, so that a record of the condition of the Sun on 279 out of the 302 days in that interval is now presented. From 1882 October 20 eight-inch photographs were taken in India, and for the measurement of these a special micrometer has been ordered of Messrs. Troughton and Simms by the Solar Physics Committee. The Solar Physics Committee have undertaken the measurement and reduction of the Indian photographs taken prior to 1881 December 22.

There has been considerable magnetic activity during the year, the month of November, which was characterized by the appearance of a very large sun-spot, being particularly disturbed with remarkable magnetic storms on November 17, 19, and 20, and many interesting cases of lesser disturbance. The magnetical changes in November are so interesting in relation to the accompanying outburst of sun-spots that it has seemed desirable to have the registers for a great part of the month as well as for other days of magnetic disturbance in the year lithographed in the 'Greenwich Magnetical Results for 1882' on a reduced scale. The magnetic disturbances on October 2 and November 17 were accompanied by brilliant auroras. Particulars of magnetic disturbances have been regularly communicated to the 'Colliery Guardian' newspaper for the information of mining surveyors.

The mean temperature of the year 1882 was $49^{\circ}6$, being $0^{\circ}1$ lower than the average. The highest air temperature was $81^{\circ}0$ on August 6, and the lowest $22^{\circ}2$ on December 11. The mean monthly temperature was above the average from January to May, then below until September. In October, November, and December it differed little from the average. The mean daily motion of the air in 1882 was 306 miles, being 27 miles greater than the average. For the month of November the mean daily motion was 449 miles, being 159 miles above the average. The greatest daily motion was 758 miles on November 4, and the least 30 miles on December 11. The greatest hourly velocity was 64 miles an hour, and the greatest pressure (with the chain) 29 lbs. on October 24. During the year 1882 Osler's anemometer showed an excess of 11 revolutions of the vane in the positive direction N, E, S, W, N, if all the turnings are counted (as has been the practice in former years); or of 23 revolutions in the positive direction if the turnings which are evidently accidental are excluded. The number of

hours of bright sunshine recorded by Campbell's sunshine instrument during 1882 was 1245, which is more than 40 hours above the average of the five preceding years. The rainfall in 1882 was 25.2 inches, being very slightly above the average.

The volume of Greenwich Observations 1880 was distributed last July, and the volume for 1881 was passed for press at the beginning of April.

There has been no case of failure in the automatic drop of the Greenwich time-ball. On three days the ball was not raised, on account of the violence of the wind. The Deal time-ball has been dropped automatically at 1^h on every day throughout the year, with the exception of 5 days, on which there was failure in the telegraphic connexion, of one day when the ball was accidentally dropped 4^s too soon by telegraph signals, and of 14 days when the current was weak and the trigger was released by the attendant without appreciable loss of accuracy. On 12 days the ball was not raised on account of the violence of the wind. The Westminster Clock has maintained its high character, the error never having exceeded 4^s, and having been under 1^s on 66 per cent. of the days of observation.

The Astronomer Royal remarks, in concluding, that the restriction of the altazinuth observations of the Moon to the semi-lunation from last quarter to first quarter has enabled more attention to be devoted to equatoreal observations; and the presentation of the Lassell telescope having removed the difficulties which arose from the inadequacy of instrumental means, he expects that observations of comets, faint satellites, and other objects of interest may be undertaken with success.

REPORT OF THE PARIS OBSERVATORY.—The Report of the Paris Observatory for the year 1882, which was presented to the Council of the Observatory on 1883, Feb. 10, by the Director, Rear-Admiral Mouchez, has recently been published. The following is a summary of its principal contents:—

The Parisian astronomers found equal cause with their brethren in England to complain of the weather during the latter half of 1882. Scarcely four or five good observing nights were secured per month; in December there were only two. Nor was this the only hindrance to the work of the Observatory; for the preparations for the expeditions to observe the Transit of Venus, and the departure on one or other of these expeditions of five members of the staff, combined to diminish very considerably the number and importance of the observations made during the year. But if in this respect last year was not so fruitful as former years have been, it has been signalized by the important improvements made in the Observatory itself—by the extension of its grounds, by the installation of the “equatoreal coude,” and by the construction of subterranean chambers for the study of terrestrial physics. The climate of Paris is so often cloudy, that the last-named depart-

ment has been added to the work of the Observatory, partly in order that its staff may have sufficient employment.

The revision of Lalande's catalogue has been carried so far, that the printing of the '*Catalogue général de l'Observatoire de Paris*,' which will include more than 40,000 stars, is already commenced. It will be completed in eight volumes, of which it is hoped that two may be published each year, or, at all events, four in three years. In connexion with this work, 110,000 observations have been made during the last four years, 27,095 of which were made in 1882.

Under the head of equatoreals, the new "equatoreal coudé" (described in the '*Observatory*,' No. 70, p. 62) takes a prominent place. This important addition to the instruments of the Observatory is due to the liberality of M. Bischoffsheim. The Brothers Henry have continued their observations relative to their ecliptic charts, Charts Nos. 12, 19, 37, 48, and 67 having been completed. These two astronomers had intended to have observed the Transit of Venus from the Pic du Midi; but they were unable, despite their utmost exertions, to force their way to the summit. They set up their instruments, however, at an elevation of 7600 feet, but without meeting with the reward the exertions deserved, for the day of the Transit was cloudy. M. Thollon paid his usual summer visit to the Observatory; but the weather was so cloudy that he was only able to adjust his instruments, and had to leave Paris without obtaining a single observation. M. Egoroff continued his researches on the telluric lines of the solar spectrum during the months of July and August. His results have been already noticed in the '*Observatory*.'

The meteorological observations, since the separation of the special Meteorological Service from the Observatory, have been carried on mainly for the purpose of determining the different corrections for the astronomical observations. The captive balloon, designed for the study of the state of the atmosphere at a considerable elevation above the Observatory, has been employed on a few occasions. These first trials show that it cannot be used when the wind is travelling more rapidly than four or five metres a second; but this limitation will not diminish its usefulness, as it is in calm weather that those great and frequent inversions of the law of decrease of temperature take place which most seriously affect astronomical refraction, and which it is proposed to study by its means.

The new subterranean chambers for the magnetic observatory have been constructed under the best possible conditions of isolation and stability. The outer walls, which are 1·8 metre thick, and quite damp-proof, enclose a rectangular area, 40 metres in length, from east to west, and 14 metres in breadth. The chambers, of which four are 5 by 4 metres, and two 6 by 5 metres, are separated from the outer wall by a gallery 2 metres wide. Their walls are 0·8 metre thick. These chambers are 3·65 metres in their greatest height, the vaulted roof is 1 metre thick, and is covered

by a layer of earth 2 metres in depth. The surface of the soil again is guarded as far as possible from the action of the Sun or of frost by groups of shrubs or by grass.

The republication of the back volumes of the observations has been completed, the volumes for 1872 and 1873 and also for 1879 having been published during the year. Volume xvi. of the '*Mémoires*' has also been published, and the printing of Volume xvii. has been brought into a forward state. The first volume of the '*Catalogue général*,' already alluded to, will appear in the course of next year. This catalogue will include all the stars observed at Paris with different meridian instruments in the 45 years from 1837 to 1881 inclusive, and will be founded on 350,000 observations. The positions are referred to three principal epochs,—1845.0 for the years 1837–53, 1860.0 for the years 1854–67, and 1875.0 for the years 1868–82. The constants employed are those given by Le Verrier in Volume ii. of the '*Mémoires*,' the coordinates of the fundamental stars having been determined by their aid. The entire work will be divided into two parts, which will be published simultaneously—the first part forming the catalogue properly so-called, the second giving the details of the observations from which the mean places of the Catalogue are concluded. Each part will be published in four volumes.

The construction of the great telescope of 16 metres is still going steadily forward. The dome, which will be the largest yet constructed, being 20 metres in diameter, will be floated by means of an annular caisson plunged in a receptacle of the same shape, which will be filled with some liquid not liable to freeze, such as water containing chloride of magnesium.

It is proposed to set up a good equatoreal on the top of the Pic du Midi by the side of the meteorological observatory of General de Nansouty. This veteran observer states that the stars often shine with such brilliance at this elevation that it is possible to read by their light and that of the Milky Way. The Brothers Henry in this clear air could see Venus with the naked eye in full daylight, when only 3° or 4° from the Sun, two days after the Transit, and fifteen or sixteen Pleiades can be seen with the naked eye. Such a position would be invaluable for many species of work, particularly for such a research as that originated by Dr. Huggins, of photographing the corona; and when bad weather puts a stop to observations at Paris, two or three astronomers could be sent to make use of the brilliant sky of the Pic du Midi.

The report concludes with an outline of the work designed to be under taken in the current year. The observation of the last series of Lalande's stars, and the publication of the first portion of the Catalogue, are mentioned first. Then follow the equatorials, amongst which is noticed the setting up in the east tower of a new telescope of 14 inches aperture belonging to the Bros. Henry, and, until the great telescope is completed, the most powerful instrument in the Observatory. The captive balloon will be used in observations of the state of the atmosphere at a height of 400 or 500 metres; and

the apparatus for the investigation of slight changes in the soil and in the position of the vertical is completed, and will be set up in a gallery of the catacombs under the Observatory, 27 metres below ground, the new subterranean chambers, mentioned above, being too much affected by neighbouring traffic for it to be installed in one of them.

An Appendix gives specimens of the method of arranging and printing the two parts of the 'Catalogue général.'

THE TOTAL SOLAR ECLIPSE OF 1883, MAY 6.—So far as we have yet learned, the various astronomers who went out to Caroline Island to observe this eclipse would seem to have met with a success which will fully repay them for having undertaken so long a journey. Much anxiety was felt that the weather should be fine on the day of the eclipse, for from the nature of the case it was not possible to provide against a local clouding by scattering the observers. Fortunately, however, the weather seems to have been all that could have been wished; and although the telegrams yet received have been necessarily very brief, they show that some most important results have been obtained. At present not much is known as to the observations secured by the American astronomers; but it can scarcely be supposed that they have been less successful in their own field than the English and French observers have been in theirs.

The anticipations founded on the observation of past eclipses as to the brightness and extension of the corona at a time of maximum solar activity would seem to have been fully justified by the observations, for Reuter's telegram reports that "the corona extended over a distance of two diameters from the Sun," and that "the light during the middle of totality was equal to that of the full moon."

Several photographs of the corona have been obtained, not only by the English observers, but also by Dr. Janssen. Unfortunately the pictures taken with the Dallmeyer photoheliograph (constructed originally for the 1874 Transit of Venus), and which would have given an image of the dark moon four inches in diameter, would seem to have not been altogether satisfactory. The photographs with the small English photoheliograph are stated to be good. M. Janssen's photographs include the regions round the Sun for a distance of 15° , in order that any intra-Mercurial planets might be detected, if any such were present. MM. Palisa and Trouvelot, however, who devoted themselves to the search for these objects, were unable to discover any.

Amongst the most important spectrum photographs secured must be reckoned the one with the integrating spectroscope, to which reference was made in the 'Observatory,' p. 194. The attempt made to secure a continuous record of the changes in the spectrum would seem to have been perfectly successful, and the apparatus has caught and registered the momentary flashing out of the multitudes of short bright lines that appear at the instant

when the Moon has just hidden or is just about to unveil the body of the Sun. These bright lines, first seen by Prof. Young in 1870, and frequently noticed in observations of subsequent eclipses, have therefore now for the first time been photographed. It cannot be doubted that this photograph will give most interesting and important information, and a similar apparatus to that by which it has been secured will probably be employed in future eclipses.

With regard to the other instruments, the spectroscopic cameras seem to have been very successful on the whole, but the results from the gratings, and especially from the Rowland grating, are not satisfactory. The lines seen in the coronal spectrum prove to be principally those of hydrogen, a most important observation, since in the eclipse of 1878, when the Sun was at its minimum activity, the hydrogen lines were seen with great difficulty and the continuous spectrum proved the most striking feature.

MR. ALBERT MARTH has succeeded Dr. Dobereck as astronomer at Col. Cooper's Observatory, Markree, Ireland.

WE deeply regret to announce the deaths of General Sir Edward Sabine on June 26, and of Mr. William Spottiswoode, President of the Royal Society, on the following day. Mr. Spottiswoode's death will be felt to be a heavy loss, not only in this country, but also by scientific men in all lands.

Astronomical Memoranda, 1883, July.

Sun. July 1, sets $8^h 18^m$, rises $15^h 49^m$; July 11, sets $8^h 13^m$, rises $15^h 59^m$; July 21, sets $8^h 3^m$, rises $16^h 10^m$; July 31, sets $7^h 47^m$, rises $16^h 26^m$.

Equation of Time:—Sun after Clock, July 1, $3^m 30^s$; July 11, $5^m 11^s$; July 21, $6^m 8^s$; July 31, $6^m 9^s$.

Sidereal Time at Mean Noon:—July 1, $6^h 36^m.8$; July 11, $7^h 16^m.2$; July 21, $7^h 55^m.6$; July 31, $8^h 35^m.1$.

<i>Moon.</i>	<i>rises.</i>			<i>sets.</i>			<i>rises.</i>	
	<i>h</i>	<i>m</i>		<i>h</i>	<i>m</i>		<i>h</i>	<i>m</i>
July 1..	14	0	July 11..	10	53	July 21..	8	22
2..	14	54	12..	11	16	22..	8	49
3..	15	57	13..	11	44	23..	9	15
4..	<i>sets.</i>		14..	12	15	24..	9	41
5..	8	26	15..	12	52	25..	10	9
6..	8	58	16..	13	36	26..	10	41
7..	9	23	17..	14	28	27..	11	17
8..	9	47	18..	15	30	28..	11	58
9..	10	10	19..	<i>rises</i>		29..	12	48
10..	10	30	20..	7	51	30..	13	46
						31..	14	49

New Moon, July 4, $3^h 4^m$; First Quarter, July 11, $19^h 49^m$;
Full Moon, July 19, $15^h 31^m$; Last Quarter, July 26, $12^h 13^m$.

The Moon is in conjunction with Saturn, July 1, 11^h; with Venus, July 2, 11^h; with Mercury, July 2, 12^h; with Jupiter, July 4, 5^h; with Saturn a second time, July 28, 22^h; and with Mars, July 29, 8^h.

Mercury is a morning star in the early part of the month. He is at greatest elongation, 21° 26' W., on July 1, 22^h, and in superior conjunction with the Sun, July 29, 11^h. He is in conjunction with Venus, July 3, 18^h, and again July 8, 4^h, and with Jupiter, July 19, 22^h.

Venus is a morning star, passing from Taurus into Gemini about July 12. It is in conjunction with Jupiter, July 26, 1^h, Jupiter being 0° 10' S. of Venus. Diameter:—July 1, 10''·6; July 31, 10''·0. Illuminated portion of disk, 0·949 on July 15.

July 1, R.A. 5^h 5^m·4, Dec. 22° 4' N., tr. 22^h 30^m, rises 14^h 23^m
31, 7 43 '7, 21 47 N., 23 10 15 6

Mars is a morning star in Taurus, in conjunction with Saturn, July 20, 1^h. Diameter:—July 1, 5''·8; July 31, 6''·1. Illuminated portion of disk, 0·923 on July 15.

July 1, R.A. 3^h 25^m·5, Dec. 18° 7' N., tr. 20^h 48^m, rises 13^h 7^m
31, 4 52 '6, 22 20 N., 20 17 12 8

Jupiter is in conjunction with the Sun, July 5, 3^h.

Saturn is a morning star in Taurus.

July 1, R.A. 4^h 12^m·4, Dec. 19° 20' N., tr. 21^h 32^m, rises 13^h 44^m
31, 4 25 '1, 19 48 N., 19 47 11 55

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	
June 28	37''·73	16''·27	25''·09	10''·82	15''·0
July 18	38 '56	16 '76	25 '64	11 '15	15 '4

The south side of the rings is visible, the elevation of the Earth above their plane being 25° 32' S. on June 28, and 25° 46' S. on July 18, and of the Sun 24° 55' S. and 25° 2' S.

Occultations.

G. M. T.			G. M. T.		
h	m		h	m	
July 14	11 11 28	Libræ	July 21	14 14 c'	Capricorni
		Oc. D. 89°.			Oc. D. 151°.
	14 12 22	Libræ		14 41 c²	Capricorni
		Oc. R. 257°*.			Oc. D. 81°.
	17 11 31	B. A. C. 6081		15 14 c'	Capricorni
		Oc. D. 120°.			Oc. R. 259
	12 41	B. A. C. 6081		15 41 c²	Capricorni
		Oc. R. 254°.			Oc. R. 329°.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

* Star below the horizon.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 76.

AUGUST 1.

1883.

William Spottiswoode.

It is but fitting that we should express our deep sense of the loss which astronomy, in common with other sciences, has suffered by the death of the late President of the Royal Society, though we may well leave to others a detailed enumeration of the important contributions to mathematics and physics which constituted Spottiswoode's special works.

William Spottiswoode was born in London in 1825, was educated at Eton and Harrow, and proceeding to Balliol College, Oxford, took a first-class in Mathematics in 1845, subsequently winning the Junior and Senior University Mathematical Scholarships. He was soon after called on to succeed his father in the management of the business of the Queen's printers, but he still found leisure to pursue his favourite studies. His earlier papers were almost entirely on questions of pure mathematics; in later years he took up the subject of polarized light, an experimental work in which he had the sympathetic cooperation of his wife, and recently he has, in conjunction with Mr. Moulton, investigated the phenomena of the electrical discharge.

But the life of William Spottiswoode is not to be measured merely by the nature and extent of his scientific researches, important and valuable though they were. The influence for good which he exercised in the scientific world and the activity which he called forth by his ready sympathy with every earnest worker were by no means confined to the special branches of science which he had made his own. His administrative qualities were ungrudgingly placed at the service of every good work in science, and his time was largely devoted to work in Council or Committee. He held the office of Treasurer of the British Association from 1861 to 1874, of the Royal Institution from 1865 to 1873, and of the Royal Society from 1871 to 1878; he was appointed Secretary of the Royal Institution in 1871, President of the British Association for 1878, and President of the Royal Society in 1879. In the last-named office he has done much to promote astronomy by

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the interest which he took in the late Transit of Venus, in Eclipse Expeditions, and in the Royal Observatory, Greenwich, of which he was, for many years, an official visitor, and since 1879 Chairman of the Board of Visitors, his last official act being to take the chair on the occasion of the last Visitation, though then suffering from the illness which so soon proved fatal.

William Spottiswoode was buried in Westminster Abbey on July 5, near the grave of his ancestor Archbishop Spottiswoode, the honour of a public funeral being accorded to him in recognition of the universal esteem in which he was held by all who knew the character of his life and work.

Sir Edward Sabine.

WE briefly announced the death of this veteran of science in our last number, but his services in indicating the connection between terrestrial magnetism and astronomy call for some further notice of his life and labours. He was born in Dublin ninety-five years ago, and after a military education at Marlow and Woolwich, obtained his commission in the artillery at the age of fifteen, becoming captain ten years later in 1813, when he saw active service in the American campaign. His scientific career may be said to have begun in 1818, when he was appointed astronomer to the Arctic expedition under Sir John Ross. Next year he accompanied Parry's expedition to the Arctic regions, and in 1821-23 he made an important series of magnetic and pendulum observations on the coasts of Africa and America near the equator, and in Greenland, Spitzbergen, and Norway, in the 'Pheasant' and 'Griper.' In 1825 he took part in the determination by rocket-signals of the difference of longitude between Greenwich and Paris, and he subsequently determined the differences in the lengths of the seconds' pendulum and of the Earth's magnetic force at these two observatories. We cannot attempt to do more than allude to the numerous important contributions on the subjects of terrestrial magnetism and pendulum observations which Sabine made during upwards of fifty years—the record of them is to be found in the Royal Society's Catalogue of Scientific Papers. It should, however, be specially mentioned that the establishment of the colonial magnetical and meteorological observatories at Toronto, St. Helena, the Cape, and Tasmania was due to his exertions, and that the observations thus made were carefully discussed by him, these observatories being, for many years, under his control. In connection with the general scheme of magnetic observation urged by Sabine, Capt. James Ross was sent out with the 'Erebus' and 'Terror' to make a magnetic survey of the Antarctic regions, and to plant the southern magnetical observatories.

In 1852 Sabine was led, from his discussions of magnetic observations, to infer a correspondence between magnetic changes and

Sun-spots—a most important fact, which has been abundantly confirmed by subsequent observation.

Sabine was elected F.R.S. in 1818, was awarded the Copley Medal in 1821, and a Royal Medal in 1849, and was elected President of the Royal Society in 1861, an office which he held for ten years.

Diameter of Uranus.*

WITH reference to an article by Prof. Safarik on Uranus, in 'Astr. Nach.' No. 2505 †, I beg to communicate herewith the result of a series of measures for obtaining the two diameters of this planet, made by me with the 10-inch refractor of this Observatory in the spring of 1881.

The diameter in the direction of the parallel was measured on nine evenings between the 14th of February and the 12th of April, using a power of 450, and taking 20 measures on each occasion. From these there resulted :—

$$d = 4''.015 \pm 0''.044,$$

where the latter number is the mean error of the result, deduced from the agreement of the determinations on the different evenings.

The diameter taken in a direction perpendicular to this or in that of a circle of declination, from similar measurements obtained on six evenings, between the 11th of March and 12th of April, is

$$d = 3''.989 \pm 0''.025.$$

Both diameters are reduced to a distance of the planet, the logarithm of which is 1.28291.

No ellipticity of the planet is perceptible from these measurements. According to the position of the orbits of the satellites, the equatoreal diameter of the planet, at the time of my observations, was nearly parallel to the circle of declination passing through the centre of the planet. This diameter, as resulting from the measures, is apparently even a trifle smaller than that perpendicular to it; but of course the difference is less than the probable error of the observations.

Geneva, 1883, June 22.

M. WILHELM MEYER.

The Great Comet of 1882.

OBSERVATIONS of this comet were made at the Kempshot Observatory in April and May, when its distance from both the Sun and the Earth had reduced it to a very faint nebulous spot of light. Under these circumstances extreme accuracy was impossible; and the observations were made by first bringing the comet to the centre of the field of view and reading the circles, and then a star whose positions are given in the 'Nautical Almanac'; the observa-

* 'Astronomische Nachrichten,' No. 2524. (Translation).

† Translated in the 'Observatory' No. 74, p. 182.

tions were then corrected for refraction, instrumental errors, and aberration—parallax being insensible. The last observation was made on May 6th, 1883, two hundred and thirty-one days after the perihelion passage! and consequently these observations can be used as a test of the accuracy of the elements of the orbit, computed from observations made several months ago.

The elements deduced by Professor Frisby are given in 'Nature,' vol. xxvii. p. 227, correcting a misprint in the eccentricity according to a subsequent notice, and taking the period as 793·789 yrs. instead of 793·689 as there given; from these elements the places of the comet have been computed for the times of the Kempshot observations.

Professor Kreutz has also computed elements of the orbit; the period he found is 843 years: and positions are given in 'Nature,' vol. xxvii. p. 541, to May 1st; the table of positions has been carefully extended, so as to include the observations made on May 5th and 6th.

Kempshot Observations.

No.	Date : G.M.T.	Right Ascension.			S. Declination.
	d.	h	m	s	° ' "
1.	1883, April 26·5614	6	8	49	7 3·5
2.	" 27·5424	6	9	25	6 58·4
3.	May 5·5306	6	14	25	6 23·1
4.	" 6·5381	6	15	1	6 19·2

Places computed from elements of Prof. Frisby.	No.	Right Ascension.			S. Declination.
		h	m	s	° ' "
	1.	6	8	49	7 3·2
	2.	6	9	24	6 58·5
	3.	6	14	26	6 24·7
4.	6	15	4	6 21·0	

Places computed from elements of Prof. Kreutz.	No.	Right Ascension.			S. Declination.
		h	m	s	° ' "
	1.	6	8	47	7 3·8
	2.	6	9	22	6 59·1
	3.	6	14	21	6 25·4
4.	6	14	59	6 21·7	

The agreement between observation and computation is very remarkable; and we may be sure that the period of the comet cannot be far from 794 years.

Four other comets are known to have elements similar to those of the comet of 1882 ; we give them all in the following table :—

No.	Perihelion passage.	Long. of Perihelion.	Long. of Node.	Inclin.	Perihelion distance.	Eccentricity.	Authority.
I.	B.C. 370 (winter).	0 1 180 ?	0 1 300 ?	0 1 30 (above)	{ very small 0°0047	1°0000	Hind.
II.	A.D. 1668 Feb. 29	277 2	357 17	35 58	0°0047	1°00000	Henderson.
III.	1843 Feb. 27	278 39	361 12	35 41	0°0055	0°99989	Hubbard.
IV.	1880 Jan. 27	277 23	358 23	36 42	0°0067	1°00000	Tebbutt.
V.	1882 Sep. 17	276 25	346 1	38 0	0°0078	0°99991	Frisby.

The direction of the motion of all these comets is retrograde, or opposite to the motion of the planets. With regard to the longitude of the perihelion and the node of comet I., we must add about 31° on account of precession in order to compare it with comets III., IV., and V.; and similarly we must add $2^\circ 47'$ to perihelion and node of comet II.

Assuming a real connection among all these comets, it becomes highly probable that comet I. has re-appeared at a late date, especially as that comet is said to have separated into two parts. If therefore we divide the interval between I. and V. by 3, we obtain a period of 751 years, which agrees very closely with the period found by Prof. Frisby. In like manner we may find periods for the other comets, and then the intermediate dates when they should have re-appeared. The following table gives the dates of theoretical re-appearance; the historical notes were taken from Chambers' 'Descriptive Astronomy.'

No.	Year, A.D.	Historical Notes.
II.	310	None.
III.	368	"
IV.	381	"
V.	382	"
II.	989	None which can apply to this comet.
III.	1106	A splendid comet appeared this year. It was first seen on Feb. 4, within $1\frac{1}{2}$ feet (?) of the Sun between the 3rd and 9th hours of the day. . . . The comet remained visible for 7 or 8 weeks and had a tail 63° long.
IV.	1131	In Sept. and Oct. a great star appeared. (Chinese Annals.)
V.	1132	On Jan. 5th a comet was seen. (Chinese Annals.)

We have thus identified comets I. and III. with the comet of 1106 ; and, referring to a brief notice of the apparent path of the

comet of 1106, we find that it suits the orbit of I. and III. as closely as possible.

With regard to comets IV. and V. and their identity with a part of comet I., there is but little to be said at present. If they appeared in 1131 and 1132, there were 4 months between their perihelion passages; in 1880 and 1882, there were 32 months between them; and this leads us to suppose that they separated at their unrecorded appearance in 381 or 382, having then formed the second part of comet I., the first part having been already identified with the comets of 1106 and 1843.

This process of disintegration is very remarkable, and would be almost incredible if we had not had the experience of Biela's comet, and if we had not seen the process going on to a certain extent in the great comet of 1882.

MAXWELL HALL.

Kempshot, 1883, May 16.

Montego Bay, P.O.

*The Rings of Saturn **

I HAVE not failed, during the last opposition of Saturn, to follow with attention the appearance of the line discovered by Kater and Encke in the outer ring A, some observations in reference to which I had the honour, on a former occasion, of relating in No. 2430 of the '*Astronomische Nachrichten*'†. But the attempts I made during the last months of 1882 gave me no satisfactory results, as I never succeeded in obtaining a sufficiently good image. In the first months of 1883 I have been able to see the planet sufficiently well on several occasions, especially on January 4, and February 16, 17, and 20, when I could verify the line with much certainty; the result in few words has been as follows:—The position of the line on the ring was the same as in the opposition of 1881–82, and the want of symmetry which I noticed on the former occasion, and which was confirmed by the observations of Dr. W. Meyer, still continued. In the right or following ansa the line was more visible, and occupied very nearly the centre of its breadth; in the left or preceding it was more difficult to recognize, and was nearer to the outer limb. But the appearance of the line was somewhat different. It seemed broader, not so dark, diffused, and somewhat nebulous; in short a diffused shading rather than a true line.

This appearance of an ill-defined shading has been remarked already on other occasions by observers who have applied themselves to the study of this object, and seems to prove that we may not consider it as a true and permanent division like that of Cassini's. It becomes, then, so much the more important to notice, as the result of the examination of all the records accessible to me, that Kater's line has been observed as presenting almost exactly

* '*Astronomische Nachrichten*,' No. 2521. (Translation.)

† Translated in the '*Observatory*,' No. 64, p. 221.

the same appearance on the northern and on the southern faces of the ring. From the time of Kater to the present, the ring has presented its greatest opening five times, three times showing us the southern side and twice the northern. The line in question has been seen and observed with more or less attention during all five of these epochs.

Since, according to the investigations of Maxwell and Hirn, the ring must be made up of minute separate parts, moving amongst themselves in a certain degree as independent satellites, the more natural way of taking account of the general drift of the facts observed would be to suppose that the ring A may not be all of one uniform density, and that towards the middle of its breadth it may be somewhat thinner, and therefore more transparent and less bright, than towards the inner and outer limbs. In this case the changes in the appearance of Kater's line and its want of symmetry would be connected with the movement of the matter of ring A about the planet, with the form, eccentricity, and position of the apsides of the orbits described by the little particles of which the ring is supposed to be composed.

As to the variability of this phenomenon, it seems reasonable to admit it in order to reconcile the different descriptions and the different degree of facility with which it has been seen by different observers. But more numerous and more continuous observations are still necessary to remove all doubts. It is not yet certain that the difference in appearance I thought I recognized in the two oppositions of 1881-82 and 1882-83 may not depend to some extent on the great difference in the atmospheric conditions, which were highly favourable in 1881-82, and generally bad in 1882-83.

On the other hand, it may be borne in mind that a variability of similar character has been ascertained in another region of the Saturnian system, viz. in the neighbourhood of the boundary between ring B and ring C, as described by Sig. Otto Struve in this periodical No. 2498*. I am about to set forth some facts I have observed, which tend to fully confirm the observations and the conclusions of the director of the Pulkova Observatory.

I ought to remark, first of all, that all degrees of atmospheric illumination are not equally favourable for examining the details of ring C with success. During full daylight the image is too faint (at least in our telescope), in complete darkness the brightness of ring B dazzles the sight and prevents the observer from examining well the outer part of C. After various experiments I have found that the best time for studying C is the moment of sunset, or not much before. The following notes have been made under such circumstances:—

1882, Jan. 21, 4^h 46^m mean time. Just at sunset I found that two dark rifts in the form of crescents were opened in the

* See 'Observatory,' No. 73, p. 154.

grey-blue veil forming ring C, and that they were arranged exactly like those described by Otto Struve in his observations of 1851. A little fringe of the transparent veil was very clearly seen to remain attached to ring B, whilst the greater part of the veil was separated from it, and formed a distinct inner ring; that is to say ring C properly so called. At 6^h 56^m in the same evening, the air being wonderfully clear and the sky being entirely dark, the image was of extraordinary beauty, and the inner edge of the ring C towards the planet was well seen, but the two divisions were no longer visible, most probably because they were masked by the brilliancy of B.

1883, Jan. 4^h 10^m. Without my having looked for it, and whilst I was intent on other objects, the phenomenon presented itself under the form of two dark arches nearly touching ring B; they were sharply defined on the concave side, that is to say towards the planet, and diffused on the side towards ring B.

1883, Jan. 23, 4^h 38^m. Saturn was well seen in the clear air, and the blue veil likewise; but in place of the division of O. Struve, I only saw a slight increase of shade as if that part of the transparent ring were of smaller density; I could not venture to decide whether there might be an interruption of continuity precisely in that place.

1883, Feb. 16, 5^h 0^m. The Sun being still above the horizon, the ash-coloured ring was well seen, but O. Struve's division was not well defined; and I could only say that it appeared darker towards the border of B. Image very unsteady.

1883, Feb. 17, 5^h 10^m. The dark arches of O. Struve very distinctly visible, and on this occasion also bounded definitely towards C, diffused towards B.

1883, Feb. 20, 5^h 13^m. I remarked that the arches above mentioned were well seen, and that they were diffused towards C and towards B, but more so on the side towards B.

1883, Feb. 22, 5^h 20^m. Saturn was unsteady at sunset; notwithstanding this the gaps pointed out by O. Struve were seen, not, however, as real spaces, but under the appearance of a spider's web, more transparent than the dark ring.

I do not venture to decide whether the difference of the appearances here related is real, and indicates changes in the dark ring, or if it is due to the atmospheric conditions. I have examined Saturn many times, especially in January 1882, with the finest definition, and towards sunset, without it happening that I noticed anything as to the relation between rings B and C. And here again I do not venture to say whether this happened from the absence of O. Struve's division, or from want of sufficient attention on the part of the observer engaged in other researches.

All the preceding observations have been made with our refractor of 8 inches by Merz, using powers of 322 and 417.

Royal Observatory of Brera, Milan,
1883, June 3.

G. V. SCHIAPARELLI.

The Armagh Observatory.

DR. J. L. E. DREYER, who was appointed Astronomer to the Armagh Observatory, has recently prepared, by order of the Board of Governors and Guardians, a short historical account of that institution, of which we give the following abstract.

The Armagh Observatory owes its foundation to Richard Robinson, Archbishop of Armagh, the great benefactor of the city of Armagh. The Archbishop had, in 1773, founded and endowed a Public Library in Armagh, and was anxious to assist in founding a University for Ulster, a project, however, which was not carried out. It was probably in connection with this plan for a University that he conceived the scheme of the Observatory, but he was also influenced by the circumstance that there was in his diocese an amateur astronomer who was both able and willing to undertake the charge of the new institution. This was Dr. James Archibald Hamilton, Rector of Kildress, County Tyrone, an enthusiastic observer of the heavens, and who was appointed by the Primate the first astronomer of the Observatory, 1790, July 31. On April 5 in the following year the Primate vested the management of the institution in a Board, consisting of the Archbishop, Dean, and Chapter of Armagh, as *ex officio* members, and two others to be chosen by them. He selected twenty acres of land, situated at the north-eastern outskirts of the city of Armagh, and belonging to the See of Armagh, as a site for the buildings and for the use of the astronomer, and further endowed the Observatory with the townland of Derry-naught, his own personal property. Beside this, to provide for the current expenses of the Observatory and for the salary of an assistant, he endowed it with a lease of the rectorial tithes of the parish of Carlingford, and of certain lands belonging to the See of Armagh.

A spacious dwelling-house with a dome 14 feet in diameter on the middle of the south side, and a transit room attached to the east, was erected on the top of a hill, about a hundred feet above the surrounding country and commanding, therefore, an extensive view. The Primate had intended to have fully equipped the Observatory, and several instruments were ordered at his expense. Unfortunately he died 1794, Oct. 10, and his heirs countermanded two of them, a transit instrument and a meridian circle, ordered from Ramsden, and the Observatory only received an equatoreal by Troughton, and three clocks. Of the clocks one, by Thomas Earnshaw, proved of exceptionally good quality, and has been in constant use from the time when it was first put up to the present day. It was improved by Dr. Robinson in 1830 by the attachment of a mercurial pendulum, and again in 1832 by the addition of a pair of compensating barometers in order to counteract the influence of the height of the barometer on the clock-rate. The equatoreal was mounted in the English manner, and its maker anticipated that it would yield results as accurate as those obtained by meridian in-

struments, an expectation which, it is needless to say, has not been realized. The right ascension and declination circles were each 4 feet in diameter, and the object-glass of the telescope was $2\frac{1}{2}$ in. in aperture.

In place of the transit instrument by Ramsden, Dr. Hamilton had one made by a local watchmaker, and observations of the Sun, Moon, and standard stars were commenced with it in 1793, July, and were continued until the instrument was dismantled in 1827. The equatoreal, besides being used for miscellaneous observations, was employed in the determination of the declinations of 37 standard stars, the results being published by Mr. Pond in the 'Philosophical Transactions' for 1806, pp. 453, 454. But few of the observations with the transit instrument have been published.

The equipment of the Observatory received no increase during Dr. Hamilton's life or during that of his successor, Dr. Davenport; but the third astronomer, Dr. Thomas Romney Robinson, on his appointment to the post in 1823, made it his first business to ascertain the state of the instruments, and found that the transit was not capable of furnishing any useful results, while the equatoreal, though its performance was good, could only find very limited application. The then Primate, Lord John George Beresford, on having the case represented to him, with great generosity decided that the necessary instruments should be procured without limitation of price. Accordingly a transit instrument and a mural circle were ordered from Jones, of London, and considerable additions to the buildings were made to make room for the new instruments. The transit room was continued towards the east by another meridian room (for the circle), and east of that again a tower was built, surmounted by a revolving dome, under which a ten-foot Newtonian by Sir W. Herschel was placed on a stone pier. In 1832 a computing room and library was built in the south-east corner between the dwelling-house and the passage to the transit room, and in 1848 a polishing room was built to the south of the east tower. The new transit instrument was mounted in the autumn of 1827, and the mural circle towards the end of 1831. The latter was 56 inches in diameter, and the telescopes for each had an aperture of $3\frac{3}{4}$ inches and a focal length of 63 inches. The ten-foot reflector was superseded in 1835 by one of 15 inches aperture and 9 feet focal length, made by Th. Grubb, and equatorially mounted, with clock movement, in the east dome. A duplicate mirror for this telescope was presented to the Observatory by the Earl of Rosse in 1843.

With these instruments Dr. Robinson worked most assiduously for a number of years, his chief object being to redetermine the position of the stars observed by Bradley about the middle of last century, to which, towards the close of the work, a number of other stars (principally from Lalande's catalogue) were added. Up to March 1837 all the observations were made by Dr. Robinson. He then handed over the transit instrument to the newly-appointed

assistant Mr. Neil M'Neil Edmondson, and himself continued to observe with the mural circle till 1850, in which year he engaged Mr. W. H. Rambaut as his private assistant, and handed the instrument over to him. Besides the observations for the star catalogue, Dr. Robinson also carried on several special investigations, particularly the determination of the longitude of the observatory by chronometers, rocket signals, and other methods, and a rigorous investigation of the law of refraction as deduced from observations with the mural circle. The results of these investigations were published in the *Memoirs of the Royal Astronomical Society*, or in the *Transactions of the Royal Irish Academy*, whilst the Primate defrayed the expense of the publication of the current observations for the years 1828, 1829, 1830. But as after 1832 many of the clergy of the Established Church were, in consequence of the Tithe agitation, for a time reduced to great distress, Dr. Robinson considered that there were more urgent claims on the Primate's generosity, and the publication of the Armagh observations was intermitted. At length, in 1850, the Royal Society voted a sum which, though not sufficient to defray the cost of printing the great mass of observations, rendered it possible to publish the single results of each observation for every star; and the work of preparing these for the press was at once commenced, a work which proved so onerous that it was not until 1859 that the work was published in a large octavo volume of more than 900 pages. The title is "Places of 5345 Stars observed from 1828 to 1854 at the Armagh Observatory. By Rev. T. R. Robinson, D.D., F.R.S., &c. Dublin, 1859" (lxvii and 847 pp.). This work, commonly known as the 'Armagh Catalogue,' is of great and recognized value. It is as yet the principal work produced at the Observatory; and considering the small staff attached to the institution, it remains a testimony of the untiring energy and perseverance of the late astronomer and his assistants. In 1862 the Royal Society presented Dr. Robinson with a Royal medal in recognition of the excellence of the work.

In 1840 the Observatory acquired a considerable number of old instruments formerly belonging to the private observatory of George III. at Kew, and presented by Her Majesty the Queen. Amongst these was one with which it was at first believed that good results could be obtained, viz. a zenith-sector, of 12 feet focal length and $4\frac{1}{4}$ inches aperture, and a tower was erected for it, and the sector mounted on a solid stand of cast iron. Unfortunately it was found that the combination of wood and metal in the construction of the sector interfered with its performance, and it was never used.

Dr. Robinson finding that these instruments could not be used, and that the limited aperture of the two meridian instruments placed obstacles in the way of observing the fainter stars, formed the plan of converting the mural circle into a transit circle by adding a second pier and axis, and substituting a telescope of larger

aperture; and after the publication of the Catalogue, the Primate, Lord John Beresford, again provided the necessary means, and the plan was carried out so far as the substitution of a new telescope of 7 inches aperture for the old one of $3\frac{1}{2}$ inches was concerned, but the second pier was not added. The improved mural circle was at once brought into use, and a new series of observations of stars selected from those observed by Lalande at the close of last century was now commenced; and in 1865 Dr. Robinson purchased and presented to the Observatory an electric chronograph, which has proved of great value in increasing the accuracy of the work. The observations have been continued up to the present time, when upwards of 3000 stars have been observed, the greater number from three to five times. Most of the observations have been made by the Rev. Charles Faris, M.A., L.C.E., who succeeded Mr. Rambaut as assistant in September, 1868. Dr. Robinson published in 1879, in the Transactions of the Royal Dublin Society, "Places of one thousand Stars observed at the Armagh Observatory" as a specimen of the work. The series is now being closed, and the definitive results in course of preparation for the press.

Armagh Observatory has been also a meteorological observatory for fifty years. Regular meteorological observations were commenced in 1833. In 1846 the cup-anemometer designed by Dr. Robinson, and known by his name, was put up on the roof. In 1867 Armagh was selected as one of the seven first-class meteorological stations of the British Isles, where complete sets of self-recording instruments (working by photography) should be in action day and night without interruption. A small house was built to the east of the tower, at the expense of the Board of Trade, and the work was regularly commenced in 1868 May, the observer (Mr. S. Call) having been appointed by Dr. Robinson. This department will be discontinued at the close of the present year, the Meteorological Council intending only to keep up the stations at Kew, Aberdeen, and Valencia.

The disestablishment of the Irish Church in 1869 entailed a very serious loss on the Observatory; and although the Government then stated that they "would at the proper time be quite prepared to consider any claim which might be preferred on its behalf," nothing has yet been done to repair this damage to its resources, and the Land Act of 1881 will certainly operate to cripple them yet further.

On the 28th of February, 1882, Dr. Robinson died, having almost reached the age of ninety, and having held the post of Astronomer of the Armagh Observatory for more than 58 years. "He found the Observatory almost destitute of instruments and comparatively unknown; he left it having acquired for it an honoured name by the well-planned and extensive series of observations embodied in the 'Armagh Catalogue,' as well as by his numerous other publications. Though he was, during the last twenty years of his life, unable to take active part in the observa-

tions, his enthusiasm for science was unabated to the last, as his elaborate experiments to determine the constants of the cup-anemometer, invented by himself, can testify. The results of these researches are contained in two memoirs in the 'Philosophical Transactions' for 1878 and 1880."

A few months after Dr. Robinson's death, Dr. J. L. E. Dreyer, Assistant Astronomer at the Dunsink Observatory, and editor of 'Copernicus,' was appointed to the vacant post.

With regard to the present equipment of the Observatory, Dr. Dreyer reports that the only instruments, besides the clocks and the chronograph, capable of producing good results, are the mural circle and the 15-inch reflector. Of these the former instrument is quite out of date as to construction, although in careful hands it produces star places not much inferior to those found by more modern instruments; and it may be confidently asserted that the catalogue of 3000 stars, which is now in course of preparation for the press, the Royal Society having granted the necessary funds, will be of value. Since to reconstruct it would cost nearly as much as a new transit circle, Dr. Dreyer recommends that its fine telescope should be mounted as an equatoreal, or as a meridian-zone instrument for differential observations of faint stars. The 15-inch reflector only needs polishing to become a useful instrument; and if an electric control was added to the clock movement it might become very serviceable for celestial photography. An application has been addressed to the Admiralty asking for the loan of one of the 6-inch refractors recently employed on the Transit of Venus expeditions, that it may be used for micrometric work. Unfortunately the funds of the Observatory are now barely sufficient for unavoidable current expenses, and there is no provision for improving the instrumental equipment.

There is an excellent scientific library in the Observatory, indebted to Dr. Robinson for many and most valuable gifts.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Apparent Inequality in the Mean Motion of the Moon.

SIR,—

Having given some attention to the subject of the Moon's motion, I have been much interested in a number of papers published within the last three months by Mr. Stone, in which the author maintains that the increasing discordance between the computed and observed place of the Moon, during the past ten years, is due to a change in the unit of time, caused by the substitution of Le Verrier's for Bessel's solar tables in the 'Nautical Almanac.'

I have had some difficulty in fully grasping Mr. Stone's views, owing to the absence of an explicit and numerically precise state-

ment of what, on his theory, the amount of the discordance ought to be. It is true that, in the 'Observatory,' p. 140, and the 'Monthly Notices,' p. 336 (1883, May), there is found the expression

$$0^{\text{m}}.747\ t - 1^{\text{m}}.54\ \left(\frac{t}{100}\right)^2,$$

but the epoch from which t is to be reckoned is nowhere explicitly given. It is, indeed, in each case, implied that this epoch is 1864.0, at which the new tables were introduced into the 'Nautical Almanac.' But I am forced to believe myself in error in thus interpreting Mr. Stone's language, because such interpretation implies that the results of computing from a set of tables may be a function of the use previously made of the tables. Allow me to put my difficulty into a more special form.

If, on any day in 1883, I observe the transits of the Sun and Moon over the meridian of Greenwich with a mean-time clock, whose daily rate is zero, and if I determine my clock-error and observed R.A. of Moon by computing the equation of time and sidereal time of mean noon from Le Verrier's tables of the Sun in Vol. iv. of his 'Annales,' and, proceeding by well-known methods, compare my lunar observation with Hansen's Tables, I shall find a discordance of some 12". If I inquire why this discordance, the only answer I can draw from Mr. Stone's theory is that it arises from the introduction of the tables I have been using into the 'Nautical Almanac,' 19 years ago. In this hypothetical answer it would be implied that if the tables had never been so introduced, the discordance in question would not be found. Since, by hypothesis, I make no use of the 'Nautical Almanac,' but only of the original tables, I am unable to see even the possibility of the assumed explanation.

Hansen's Solar Tables, the mean motion of which is substantially identical with Le Verrier's, have been used in the American Ephemeris since 1858. I should be much pleased to know what, on Mr. Stone's theory, are the discordances thus introduced.

It would also be of great interest to me if Mr. Stone, or some one who has mastered his theory, would give general formulæ for the differences between the Moon's apparent errors at all epochs between — 720 and the present time, according as one or another set of tables is used. I would also beg leave to inquire, in this connection, what the discordances in question would have been had the Bessel-Carlini tables been used in the 'Nautical Almanac' until the present time.

SIMON NEWCOMB.

Neuchâtel, Switzerland, 1883, July 11.

Biela's Comet.

SIR,—

In preparing a lecture upon Biela's Comet, for delivery at Gresham College on the 24th of May last, I noticed that Dr. Hind, in his well-known work upon Comets, spoke of its duplication as

having been first observed on Dec. 29, 1845. I also found that the same date was given in Humboldt's 'Cosmos,' and misquoted as Dec. 27 in Arago's 'Popular Astronomy' (English edition); but that, in the great majority of astronomical treatises, in fact I may say almost universally, it was stated that the duplication was not observed until Jan. 13, 1846, on which day Lieutenant Maury perceived it at Washington. It is also generally believed that the well-known observation of the phenomenon by Prof. Challis at Cambridge, England, on Jan. 15, 1846, was only two days later than the first observation in America. Even in such books as Cooper's 'Cometic Orbits' and Prof. Newcomb's new edition of his most valuable book upon 'Popular Astronomy,' no reference is made to the earlier date mentioned by Dr. Hind and Humboldt. It therefore occurred to me to make such a search as the short amount of time at my disposal permitted, in the library of the B. A. S., for a record of the observation of Dec. 29, 1845.

In Houzeau's 'Vade Mecum' I found it assigned to Messrs. Herrick and Bradley of New Haven, U. S. I also found an important discussion of the comet by Prof. Hubbard in Gould's 'Astronomical Journal,' vol. iv. p. 3, but I did not discover any original account of the observation in question.

My friend Mr. Lynn has, however, very kindly continued the search, and has found that there are other interesting articles by Prof. Hubbard in Gould's 'Journal,' vol. vi. In one of these, p. 130, Prof. Hubbard refers to the 'American Journal of Science,' 2nd series, vol. i. p. 293, for a record of the observation made by Herrick and Bradley, which is described as having been made with the 5-inch refractor at Yale College, New Haven, U. S.

It is, however, evident from a paper upon the comet by Lieut. Maury, in the *Astr. Nach.*, that his subsequent announcement of the duplication was made in entire ignorance of this previous observation.

Prof. Hubbard, in his final discussion of the path of the comet in Gould's *Astr. Journal*, vol. vi. p. 160 (published in 1860), comes to the important conclusion that in all probability the actual separation of the secondary comet occurred as early as Nov. 1844.

In Gould's 'Journal,' vol. iv. p. 3, he also mentions another very interesting fact, viz. that the positions of the comet observed by Prof. Challis on Dec. 1 and 3, 1845, correspond much better with calculated places for the *secondary* than for the *primary* nucleus. And this led him to suggest, in vol. vi. p. 130, that possibly at that date such alterations in the comparative brightness of the two bodies as were afterwards noticed both during the apparition of 1845-6, and still more decidedly near to the time of perihelion passage in 1852 (when the secondary nucleus suddenly became very bright and the primary quite faint), may have led Prof. Challis to have observed the secondary comet on those dates without noticing the primary.

It is certainly curious that Prof. Hubbard's interesting investi-

gation seems to have been so little noticed by many subsequent writers, and that America should have consequently lost the credit which, in this instance, as on many other occasions since, it has so well deserved.

If you think that this note, upon so comparatively trivial a matter, is worthy of insertion in the 'Observatory,' kindly allow me a little additional space to mention that my thanks are due not only (as I have already stated) to Mr. Lynn, but also to Mr. Wesley for much assistance in referring to various volumes, and to the Librarian of the Royal Society, who allowed Mr. Lynn to consult the volume of the 'American Journal of Science,' which is unfortunately wanting in the R. A. S. Library.

The recent breaking up of the nucleus of the Comet 1882 *b* may perhaps make this reference to the case of Biela's Comet more interesting than it otherwise might be at the present time.

Barham, 1883, June 11.

E. LEDGER.

P.S.—Some of your readers may be glad to know that there is an article full of information upon Biela's Comet by Mr. Lynn, in the 'Intellectual Observer,' vol. xi. p. 208.

Also that the passage referred to by Prof. Hubbard in the 'American Journal of Science,' 2nd series, vol. i. p. 293, speaks of the comet as follows:—

"During its present return it has exhibited a very remarkable appearance. When first observed through the 5-inch refractor at Yale College, Dec. 29, 1845, the comet was seen attended by a faint nebulous spot preceding, estimated to be rather more than a minute of space distant from its brightest point. The few subsequent observations which the clouds and the moonlight permitted here before the middle of January, showed this secondary comet to be brightening faster than the principal and slowly separating from it. This surprising phenomenon was first publicly announced in this country by Lieut. Maury, of the Washington Observatory."

I understand from Mr. Lynn that the names of Messrs. Herrick and Bradley of Yale College are not mentioned in the 'American Journal of Science,' but are given by Prof. Hubbard, who says, in Gould's 'Journal,' vol. vi. p. 130:—"We find that, although the companion was first recognized by Herrick and Bradley at New Haven (Am. Journ. Science, 2nd series, i. p. 293), it was not again seen until Maury observed it on the 13th of January."

It may, however, be noticed that the quotation in the 'American Journal' almost implies that, notwithstanding clouds and moonlight, some other observations of the two nuclei were obtained between Dec. 29 and the middle of January.

Flamsteed and Morin.

SIR,—

Probably few anecdotes in the history of astronomy are better known to general readers than that related by Flamsteed,

respecting the foundation of the Royal Observatory being hastened, if not occasioned, by the application of the Sieur de St. Pierre to Charles II. (through the Duchess of Portsmouth) for a reward for discovering a method of finding the longitude at sea, and Flamsteed's own decision on its impracticability until the motions of the Moon and the places of the fixed stars had been determined with much greater accuracy than was then possible. But it is not easy to understand the exact meaning of one of Flamsteed's expressions to St. Pierre. He says that he told him, after first proving to him how incompetent a calculator he was, and pointing out that, independently of this, his method was inapplicable in practice, to go to his own countryman, Morinus, who would instruct him in a better method than his own, and not to return to the king of England until he had done so. Of course, the general force of this recommendation was, in vulgar English, to bid him go to Jericho. But surely Flamsteed could hardly have been ignorant (though he does not refer to it) that Morin had, in 1634 (41 years before the application of St. Pierre to Charles II.), submitted a plan similar in principle to Cardinal Richelieu, and that a committee appointed by the latter came to the same decision as Flamsteed concerning St. Pierre's proposal, that such a method was of no practical use in the existing state of astronomical knowledge. To me it seems exceedingly likely that St. Pierre was aware of what had taken place with regard to Morin; that, in fact, he had stolen the principle from the latter (who, although he deserves all the contempt that Mädler pours upon him* for prostituting astronomy to the purposes of that mass of imposture and delusion which has robbed our science of its more appropriate name of astrology, was a good mathematician for those times) and interpreted Flamsteed's last remark into the imputation that he was in point of fact found out. Flamsteed says that he heard no more of him afterwards; but he certainly did *not* go to Morin, for the best of all reasons—Morin having died more than eighteen years before, on the 6th of November, 1656.

Yours faithfully,

W. T. LYNN.

Blackheath, 1883, July 16.

Abnormal Appearance of the Lunar Crescent.

SIR,—

Observing the Moon at 18^h 15^m Nov. 8, 1882, its age being 27.9 days, I was surprised to find that the "line" joining the cusps did not pass as usual through the Moon's centre, but a little to the E. of it; in other words, the exterior circumference of the crescent was *less* than a semicircle.

It is well known that both Schröter and Gruithuisen frequently traced the "horns" of the crescent for some little distance beyond a semicircle†; but that the "cusps" should be observed not

* Mädler, 'Geschichte der Himmelskunde,' Vol. i. p. 317.

† 'Celestial Objects,' 4th ed. p. 78.

extending so far as to form the semicircle, is, so far as I am aware, one of those "things not generally known," and one, too, that cannot fail to be of interest to astronomers in general and selenographers in particular.

The foregoing observation having been made solely with the unaided eye, and not being able at the time to telescopically observe the Moon, it occurred to me that it was possible that this abnormal appearance of the lunar crescent was due to the "cusps" being such fine lines of light that the Moon's proximity to the Sun, combined with the brightness of the morning twilight, rendered them invisible to the naked eye. But on observing the Moon at 19^h on January 5th of the present year, its age being 26.5 days, I was exceedingly gratified to find the Moon presenting the same appearance as on the former occasion, the only difference being that the peculiar form of the crescent was more conspicuous.

That this was the *true* shape of the crescent, and not caused by the brightness of the twilight, I *proved* by viewing the Moon with a small terrestrial telescope of 1½-inch aperture and power 25, with which the abnormal appearance was rendered most striking.

Trusting that you will find room for this somewhat lengthy communication, which I should have sent to you before, only I wished to make some further observations before doing so; but the weather and other things have prevented me making them, and the illness of a near and dear relative has made it impossible for me to write this letter until now. I am Sir, yours faithfully,

10 Malvern Road,
Dalston, E.

B. J. HOPKINS.

*Tabular Mean Errors of the Moon's Longitude in
1880 and 1881.*

SIR,—

The Greenwich Observations for 1881 having been recently published, I have thought it would be worth while to calculate the mean errors of the Moon's longitude from the altazimuth observations (in continuation of my former papers published in Vol. xl. of the 'Monthly Notices') for the successive quarters both of that year and of 1880, and send you the result as follows:—

Period.	Mean Error of Longitude.	No. of Observations.
1880, 1st Quarter,	+ 9.08	35
2nd "	+ 10.35	50
3rd "	+ 10.49	44
4th "	+ 10.04	51
1881, 1st "	+ 9.99	42
2nd "	+ 8.24	50
3rd "	+ 10.77	43
4th "	+ 9.37	51

An inspection of these numbers at once shows that, whilst a

small increase is still manifested over previous years, the augmentation since 1876 is not considerable, and the mean error is indeed slightly less in 1881 than in 1880. I will here place in order the mean error for the years from 1876 to 1881 :—

Year.	Mean Error of Longitude.	No. of Observations.
1876.	+ 9.31	171
1877.	+ 8.25	182
1878.	+ 7.48	161
1879.	+ 9.04	155
1880.	+ 10.05	180
1881.	+ 9.53	186

I remain, Sir, yours faithfully,
Blackheath, 1883, July 25. W. T. LYNN.

NOTES.

THE SPECTROSCOPE AS AN AID TO FORECASTING WEATHER.—At the meeting of the Meteorological Society on June 20, Mr. Frederic W. Cory, M.R.C.S., F.M.S., read a paper on the above subject. He commenced by quoting from Mr. Rand Capron's pamphlet "A Plea for the Rain-band" *, a short history of the rainband, which though not the only phenomenon in the spectrum to be taken into account in foretelling weather, he yet considered to be the most important. The principal rainband is a dark shading (in spectroscopes of small dispersive power) situated on the red or least refrangible side of the D lines of the spectrum, involving them and at times increasing or decreasing in intensity, and approaching or receding from the C line according to the nearness or quantity of rain. It has been Mr. Cory's rule for the last ten months to observe towards that point from which the wind is blowing; if, he adds, one's knowledge extends to foretelling the probable direction of the wind from movements of the upper clouds or other signs in the direction from which they are coming, then so much the better will be the success with the spectroscope.

Mr. Cory then gave a number of instances during the present year in which the appearance of the rainband had furnished a forecast of rain. Thus Jan. 9 to Jan. 13, the rainband gradually increased and then remained persistently high; and as only a small amount of rain fell during these days, Mr. Cory expected a further fall, and on Jan. 15 and 16 heavy rain fell. Again, Feb. 1 to Feb. 8, the rainband was fairly marked, and on Feb. 9, 10, and 11, rain fell to the amount of 1.351 inch. The fall of rain (0.126 inch) that occurred on the early morning of March 1 was

* 'Observatory,' No. 58, pp. 43, 44.

preceded by an increase of the rainband commencing Feb. 27, neither the hygrometer, the barometer, nor any particular aspect of the sky indicating rain. Again, the heavy rains of April 28 and 29 were very clearly predicted.

The rainband does not seem to predict snow; as a rule it diminishes several days before snow, and Mr. Cory feels confident that when with a small rainband, rain apparently falls, it is in reality either melted snow or hail. The result of the observations made at 9 A.M. for four months, 120 days, was as follows:—

There were two cases of 70 per cent., and rain followed in both; four of 60 with rain in all; two of 55 per cent., and rain in each one; twelve in 50 per cent., and rain in every case; one of 45 with rain; nineteen of 40 with twelve of rain and seven without rain, of which three were followed by overcast skies; four of 35, three with rain, one without; twenty-nine of 30 with eighteen rain, two snow, and nine without; sixteen of 25 with two rain, three cold rain, snow and hail, two snow, and nine no rain; thirteen of 20, one snow and rain, three snow, and nine without rain; eleven of 15, in one of these a few drops of rain followed, the temperature in the screen at the time being 33°·5 wet bulb, 34°·9 dry bulb, one rain and snow, five snow, four none; four of 10, one rain and snow, two snow, one without; one of 40, decreasing to 30 shortly afterwards, no rain but nimbus clouds present; one of 30, increasing to 40, rain ·085 in.; one of 20, increasing to 30 per cent. at 3 P.M., slight rain, then heavy snow-fall commencing at 9 P.M.

Other spectroscopic indications according to Mr. Cory are, the appearance of the F line, the variations in the dry air-band between C and D, and the bands near C. A more curious phenomenon, one well known to spectroscopists, the apparent change in the colours of the spectrum, Mr. Cory also believes to be associated with weather changes; but this view, at least, seems to stand in need of much stronger support than has yet been given to it, since the phenomenon in question would appear to be in all probability principally if not entirely a subjective one.

In the discussion which followed the paper, it was objected to the usefulness of the spectroscope in forecasting weather, that its predictions were of a somewhat vague nature, that a dark rainband might mean rain in an hour or two, or not for two or three days, and that more definite and trustworthy indications could be obtained from the barometer, wet and dry bulb thermometer, and the other ordinary means for predicting weather. On the other hand it is not always sufficiently borne in mind that the spectroscope is an instrument which it requires some little education to be able to use efficiently, and that both patience and practice are needed before the effect of a variation in the brightness of the sky on the apparent extent and depth of the rainband can be properly estimated and allowed for.

THE TOTAL SOLAR ECLIPSE OF 1883, MAY 6.—The interest of such details of the American observations of the eclipse as have yet come to hand would seem to centre round the observations of Prof. Hastings and the interpretation which should be put upon them. Prof. Hastings had arranged his spectroscope so as to have portions of the corona on opposite sides of the Sun under examination together, and he found that at the commencement of totality the 1474 K line, to which he chiefly directed his attention, was 12' long on the east side of the Sun, but short and faint on the other. As the eclipse went on, the line from the western side developed, whilst that from the eastern shortened, until at the end of the total phase the conditions, as first seen, were entirely reversed. Prof. Hastings claims these observations as establishing a theory he has recently put forward, as to the non-solar character of the corona. According to this theory, the true corona is confined to a very narrow ring around the Sun, whilst the exterior portions are simply a diffraction phenomenon, the true corona being optically widened out by diffraction at the limb of the Moon.

Mr. Lockyer, in an article in 'Nature' for July 5, discusses these observations of Prof. Hastings, and though he does not think "that any one will go with Prof. Hastings when he proposes to abolish a true corona at the Sun," adduces many considerations as establishing the essentially dual nature of the phenomenon, and suggests not only that much of the corona as seen is simply due to atmospheric glare, but that the 1474 K line when detected by the spectroscope at great distances from the Sun owes its apparent presence in those outer regions to the same cause. We venture, however, to think that the evidence for the reality of the great extension of the corona is too strong to be lightly set aside, and that it is far more probable that the rays seen by Prof. Newcomb, extending six degrees from the Sun, were true solar appendages, than that the only really existent corona is one limited to four or five minutes of arc in depth. And although there are instances when, either by the eye or the photographic plate, the coronal lines have been traced right across the dark body of the Moon, we do not think that the theory of atmospheric glare can be quite satisfactorily held to account for every case in which the 1474 K line has been seen at a great distance from the Sun. Notwithstanding Prof. Hastings's observation, then, we are disposed still to regard the *apparent* corona as principally and essentially a true solar appendage.

The search for Vulcan which Prof. Holden conducted had, like that conducted by the French observers, a purely negative result. Both French and American spectroscopists concur in reporting that no dark lines, except D, could be detected in the coronal spectrum. The corona is reported as having been bright with five well-defined streamers. The chromosphere was unusually quiescent.

Further information has also been received as to the success of

the English observers. Mr. Lawrance was able to examine the corona with a finder telescope, and detected a large amount of delicate detail, especially near the preceding limb of the Moon. Examining the spectrum with the prisms from a pocket spectroscope, the green ring corresponding to 1474 K was very distinct and seen even after totality. The red and yellow rings corresponding to C and D₃ were also seen, though but faintly; but F, though specially looked for, could not be detected.

The photographs taken with the small photoheliograph were very successful, and the one to which an exposure of two minutes was given shows as much as those which M. Janssen exposed during the whole of the totality, which lasted five minutes twenty-five seconds. The plates exposed in the large photoheliograph all show signs of slight shifts. Still it is believed that by combining all the nine photographs a complete representation of the whole of the corona may be secured.

With the first order grating and with the dense prism spectro-scope photographs showing bright lines were obtained just before and immediately after totality, the grating showing H and K, and the spectroscope showing H, K, *h*, *f*, and F.

A good photograph extending from the ultra-violet to the green was obtained with the slit spectroscope. This spectrum is mainly continuous, but H and K are very strong. The hydrogen lines, 1474 K and *b*, are also shown, and it is noteworthy that the spectrum is not exactly the same on both sides of the Sun. The plate driven by clockwork recorded nothing during totality, but, as stated before, caught the flashing out of bright lines at the beginning and end of totality.

The plate on which the photograph of the red end of the spectrum was obtained was unfortunately torn during development, and completely destroyed.

The meteorological observations made during the eclipse showed that there was a slight rise in barometric pressure and in humidity, but a very marked fall in temperature, which fell even to nightly values. No change in the direction or velocity of the wind took place.

On May 4, when the photographers had hoped to have exposed some trial plates, a heavy rain-storm fell and spoilt the dark room, and though the damage was repaired as speedily as possible, no plates were exposed until the eclipse. The morning of the eclipse the weather was still unsettled, but fortunately the clouds dispersed and the sky became moderately clear in good time for the eclipse to be observed.

THE SUN-PILLAR AND THE ZODIACAL LIGHT.—‘Nature,’ Vol. xxvii. No. 703 and the following numbers, contained several letters describing the unusual phenomenon of a Sun-pillar, which was witnessed, soon after sunset on April 6, by persons as far apart as Bath, St. David’s, Dolgelly, Hull, and Hertfordshire. It was described as follows by Major Gibney, ‘Nature,’ Vol. xxvii. p. 605:—

"The sunset was brilliant and cloudless, but from the horizon to about 25° in height, immediately above the spot where the Sun had disappeared, there appeared a ray of light of great beauty and extreme brilliancy; its centre, a delicate rose colour, graduating to the edges into the purest gold. This single ray was perpendicular, and appeared to be little, if at all, affected in its brilliancy by the approaching dusk of evening, but continued to exhibit itself with little-diminished brilliancy for nearly half an hour, finally disappearing with the twilight."

Various suggestions were made as to its origin. Observers familiar with the appearance of the zodiacal light were agreed that the two phenomena were quite distinct. Mr. G. J. Symons believed that it was merely a portion of a halo passing vertically through the Sun; Mr. Newall that it was caused by reflection from ice-crystals. A similar phenomenon was observed by Mr. Maxwell Hall in Jamaica at sunset on May 15, who writes as follows on the subject in the Weather Report for May of the 'Jamaica Gazette':—

"At Kempshot it appeared on May 15th, at 6^h 30^m local mean time, as a bright ray of light of a faint roseate hue, 2° in width and 30° in height above the horizon, vertical, but not passing through the Sun. A rough sketch was made at the time, and the circles of the equatorial telescope were afterwards employed to determine the azimuth of the point where the pillar cut the horizon. This was 70° from the north towards the west; and as the Sun's azimuth was 60° at the same time, the pillar cut the horizon 10° to the west of the Sun. The sketch also showed that the pillar was a little to the west of the Sun.

"Now with regard to the nature of the phenomenon, it certainly was not the usual display of Zodiacal Light. The Light is here seen to perfection: every fine night when there is no moonlight, the Zodiacal Light may be seen following the ecliptic from the one horizon to the other, with but little variation except perhaps as to the *Gegenschein*, or stronger illumination near the point in the heavens diametrically opposite to the Sun. And so clearly is it seen, that some years ago I carefully measured its breadth at different distances from the Sun, and so formed the following table:—

Angular distance from Sun.	Breadth of Zodiacal Light.	Angular distance from Sun.	Breadth of Zodiacal Light.
30°	$41\cdot4$	110°	$20\cdot3$
40°	$38\cdot7$	120°	$17\cdot8$
50°	$36\cdot1$	130°	$15\cdot3$
60°	$33\cdot4$	140°	$13\cdot0$
70°	$30\cdot7$	150°	$10\cdot8$
80°	$28\cdot1$	160°	$8\cdot9$
90°	$25\cdot5$	170°	$7\cdot6$
100°	$22\cdot9$	180°	$7\cdot0$

"From various considerations based upon the figure corresponding to these measures, I consider the Zodiacal Light a terrestrial phenomenon—rays of light are swept back from the tropical parts of the Earth by the Sun and tend to accumulate at the point in the heavens diametrically opposite to the Sun.

"If there be any truth in this theory of the Zodiacal Light, the Sun-pillar may be a strong and comparatively local development of the same Light; this is the only explanation I can give; the explanation given by Mr. G. J. Symons, the well-known meteorologist, "that it is merely a portion of a halo passing through the Sun" (Nature, Vol. xxviii. p. 7) will not apply to the Jamaica observation at all; the sky was far too pure and transparent at the time, and there was not the least trace of cirrus cloud."

THE OBSERVATORY OF MOSCOW.—The second part of the ninth volume of the '*Annales de l'Observatoire de Moscou*' has just been received. The first section contains the results of the solar observations made from 1882, April 19, when the great sun-spot was first seen at Moscow, to 1882, Oct. 5. The Sun was photographed on 74 days in this interval, and the positions and areas of the spots determined. The second section gives the results of the spectroscopic observations made on the Sun from 1881, April 17, to 1881, Oct. 19, during which period the limb was examined for prominences on 49 days, and their positions and heights duly noted. The third section, illustrated by five plates, contains Dr. Bredichin's researches on Comet 1882 b. A number of observations are brought together so as to form a tolerably complete history of the development of its various appearances; and Dr. Bredichin enters into an interesting investigation as to the values of the repulsive forces indicated by the forms presented by the tail at different dates. The fifth section contains photometric observations by M. Ceraski, the sixth observations of Jupiter made in 1881 and 1882, illustrated by 13 sketches, and the seventh contains a few observations of the places of certain comets, including some measures of the positions of the bright bands in the spectrum of comet 1881 b.

CHANGES IN THE LENGTH OF THE JULIAN YEAR*.—The '*Comptes Rendus*' of the Academy of Sciences for July 16 contains a note by M. A. Gaillot, presented by M. Tisserand, on the changes produced in the length of the Julian year by variations of the quantities on which that length depends. Referring to Mr. Stone's paper in the '*Monthly Notices*,' Vol. xliii. No. 7, in which a considerable relative influence on the value of the unit of time employed in the construction of astronomical tables is ascribed to the small difference existing in the expression for the mean motion of the Sun adopted by Bessel, and that adopted by Hansen and Le Verrier, M. Gaillot wishes to show that this influence is in reality very small,

* *Comptes Rendus*, Vol. xovii. No. 3.

and that within the limits of time for which our present astronomical tables are susceptible of presenting sufficient precision, the effect on the calculation of the theoretical positions of the heavenly bodies is almost insensible. He points out that the difference between the mean times calculated by different Solar Tables for any epoch is equal to the difference, expressed in time, between the mean longitudes of the Sun furnished by those tables. Comparing the tables of Le Verrier and Bessel, we find for the mean longitude of the Sun,

$$LV - B = +7''.39 + 0''.060216(t - 1850),$$

and consequently for the mean time

$$LV - B = +0''.493 + 0''.004014(t - 1850).$$

The error, which is $+0''.5$ in 1850, will only become $+2''.55$ after five hundred years. The present difference between the mean times $+0''.63$ can only produce, in the calculation of the theoretical positions of the Moon, a difference of about $0''.4$, whilst the ascertained difference between theory and observation exceeds $10''$. For the Sun, the difference does not amount to $0''.03$. M. Gaillot does not therefore agree with Mr. Stone as to the undesirability of the increase in the mean motion of the Sun adopted by Le Verrier. A direct verification of the above formula is found by comparing the sidereal time at mean noon given in the 'Nautical Almanac' for 1883, for 1864 January 1 (1863 December 32), with that given for the same date in the volume for 1864; the first is deduced from Bessel's tables, the second from Le Verrier's; the difference $LV - B = +0''.55$ is exactly that given by the formula if t is taken as 1864. The 'Connaissance des Temps' gives elsewhere exactly the same difference.

NEW METHOD FOR OBSERVING ECLIPSES OF JUPITER'S SATELLITES*.—M. Cornu, having regard to the difficulties at present experienced in obtaining satisfactory determinations of the times of these phenomena, and the numerous errors to which their observation is exposed, has proposed a method by which he hopes very greatly to increase the precision with which they may be observed. He suggests that the brightness of the satellite should be compared with that of an artificial satellite the brightness of which can be regulated at will; and that, further, instead of noting the time of total disappearance or first appearance near which times the light of the satellite changes but slowly in intensity, it should be principally observed when about dichotomized, as the change then takes place with the greatest rapidity. He recommends that a determination of the brightness of the field as compared with that of Jupiter be always made in addition. This he proposes to determine by a *photopolarimeter*, in principle much the same as the photometer devised by Prof. Pickering; and apparently unaware of the very considerable amount of work Prof. Pickering has actually accomplished with this instrument during the last two years in determining the light-curves of Jupiter's satellites during

* Comptes Rendus, Vol. xvi. Nos. 23 and 26.

the progress of their eclipses, M. Cornu suggests that it might be also used for that purpose.

CONNEXION BETWEEN ECLIPSES OF THE SUN AND TERRESTRIAL MAGNETISM*.—In a paper by P. Denza contained in Vol. xvi. of the 'Comptes Rendus,' page 1575, the author discusses the hitherto disputed question of the connexion between eclipses and terrestrial magnetism. He mentions that he was led to undertake the investigation on account of the occurrence of certain anomalies in the motion of the magnetic needle during the total solar eclipse of 1870, Dec. 22, in consequence of which regular observations of magnetic declination were commenced at the Observatory of Moncalieri on all occasions of eclipses of the Sun, as well as at some eclipses of the Moon. At such times the needle was observed at short intervals, as every 10 minutes, and frequently every 5 minutes, the eclipses at which observations were thus made numbering twenty, the first being that of 1870, Dec. 22, and the last that of 1882, May 17. The author finds no relation between the amount of magnetic change and the magnitude of a solar eclipse, and indeed no connexion between magnetism and eclipses; but his general conclusions are too lengthy for extract. He considers that he has sufficiently established as a physical law that the conjunction of the Sun and Moon in eclipses of the Sun, and their opposition in eclipses of the Moon, has no influence upon the magnetism of the Earth, and that there is no connexion between eclipses and terrestrial magnetism.

W. E.

YOUNG'S NAVIGATION AND NAUTICAL ASTRONOMY†.—This manual, which is No. 99 of Weale's Educational Series, sets forth, in a clear concise manner, the theory and practice of Navigation and Nautical Astronomy, and supplies the young navigator with a very complete and handy guide to his science. The definitions of the terms employed and the explanations of the principles involved are clear, and especial care is taken that the best methods of solving the various problems arising in practical work should be easily understood and followed; and the method, so excellent in a manual for beginners as this is, of providing blank forms rather than formal rules, where these would be long and tedious, has been systematically employed. The author is also careful to discriminate those cases where logarithms are of most advantage and those where ordinary arithmetic will suffice. As most of the astronomical examples in the book are adapted to the year 1858, the publishers have added in the new edition the information necessary for their reduction from the 'Nautical Almanac' for that year, and have so rendered more complete a very thorough and useful manual.

* Comptes Rendus, Vol. xvi. No. 20.

† 'Navigation and Nautical Astronomy in Theory and Practice.' By J. R. Young, Formerly Professor of Mathematics in Belfast College. New Edition, including the requisite elements from the 'Nautical Almanac' for working the Problems. London: Crosby Lockwood & Co., 7 Stationers' Hall Court, Ludgate Hill, 1882.

Saturn is in conjunction with *Aldebaran*, Aug. 12, 23^h.

Aug. 1, R.A. 4^h 25^m.4, Dec. 19° 49' N., tr. 19^h 44^m, rises 11^h 51^m
 31, 4 33 '4, 20 3 N., 17 53 10 0

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Aug. 7	39''70	17''34	26''40	11''53	15''8
27	41 '08	17 '99	27 '32	11 '96	16 '3

The south side of the rings is visible, the elevation of the Earth above their plane being 25° 54' S. on Aug. 7, and 25° 58' S. on Aug. 27, and of the Sun 25° '0' S. and 25° 17' S.

Neptune is in quadrature with the Sun, Aug. 13, 18, and stationary, Aug. 26, 4^h.

Aug. 1, R.A. 3^h 16^m.0, Dec. 16° 19' N., tr. 18^h 34^m, rises 11^h 4^m
 31, 3 16 '5, 16 19 N., 16 37 9 7

Phenomena.

G. M. T.			G. M. T.		
h	m		h	m	
Aug. 15	7 25	ρ ³ Sagittarii	Aug. 21	15 29	J. i. Sh. E.
		Oc. D. 70°.	24	10 30	B. A. C. 1206
	8 32	ρ ² Sagittarii			Oc. D. 110°.
		Oc. R. 308°.	11	21	B. A. C. 1206.
21	12 20	ε Piscium			Oc. R. 275°
		Oc. D. 184°.	28	15	4 J. i. Sh. I.
	12 42	ε Piscium		15 55	J. i. Tr. I.
		Oc. R. 223°.	29	15 34	J. i. Oc. R.

The angles are reckoned from the apparent N. point towards the right of the Moon's inverted image. EDITOR.

Publications received:—Simon Newcomb, *On Hell's alleged falsification of his observations of the Transit of Venus in 1769* (Monthly Notices R. A. S. Vol. xliii. No. 7)—Le Contre-Amiral Mouchez, *Rapport Annuel sur l'état de l'Observatoire de Paris, pour l'année 1882* (Gauthier-Villars, Paris)—*Annual Report of the Chief Signal Officer to the Secretary of War for the Fiscal Year ending June 30, 1880, Parts I. and II.* (Government Press, Washington)—*Washington Astronomical and Meteorological Observations, 1878, Vol. xxv.*—Asaph Hall, *The Parallax of α Lyrae and 61 Cygni* (Government Press, Washington)—*Jamaica Weather Report for the Month of April, 1883, also for the Month of May, 1883* (Government Printing Establishment, Kingston, Jamaica)—Isaac Sharpless, *The Latitude of Haverford College Observatory* (American Philosophical Society).

ERRATUM IN No. 75.

Page 220, line 11, for p. 574, read facing p. 755.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 77.

SEPTEMBER 1.

1883.

Professor Lemström's Auroral Experiments in Lapland.

THE scientific world has lately had presented to it several contributions upon that ever interesting subject the Aurora Borealis, among which we may particularly mention Mr. Backhouse's valuable analysis of the lines of the auroral spectrum, contained in his paper on the Spectrum of the Aurora, printed in 'Nature,' Vol. xxviii. p. 209. Probably, however, the greatest general interest has attached to certain experiments and observations made in Lapland by Prof. Selim Lemström, of Helsingfors, upon what has been by some curiously mis-called an "Artificial Aurora." The first information we had on the subject was by a telegram dated Dec. 11, 1882, from the Professor in his character of chief of the Finnish Meteorological Observatory at Sodankylä, to the effect that, having placed an apparatus ("battery" was the word used in the telegram, but evidently this did not mean the instrument electricians generally understand by that term) with condensers covering an area of 900 square metres on the hill of Orantunturi (lat. $67^{\circ} 21'$, long. $27^{\circ} 17' 3''$ E. of Greenwich), he found the cone (of the hill) to be generally surrounded by a yellow-white halo which yielded perfectly, though faintly, the spectrum of the Aurora. A subsequent telegram acquainted us of further experiments made on December 29 at Lake Enare, near Kahala, on the hill of Pictarintunturi (lat. $68^{\circ} 32' 5''$, long. $27^{\circ} 17' 3''$ E. of Greenwich), confirming the previous results obtained at Orantunturi, a straight beam of Aurora in the latter case being seen over the apparatus.

Later on ('Nature,' Vol. xxvii. p. 389), Prof. Lemström gave by letter some details of the apparatus employed; and in April and May last we have also, in 'Nature,' Vol. xxviii. pp. 60, 107, and 128, direct from the Professor, full particulars not only of the apparatus and the observations in question, but of other auroral phenomena observed by him in those regions. At the outset it was no doubt felt that the accounts transmitted must have been necessarily abridged and imperfect; and with the exception of the

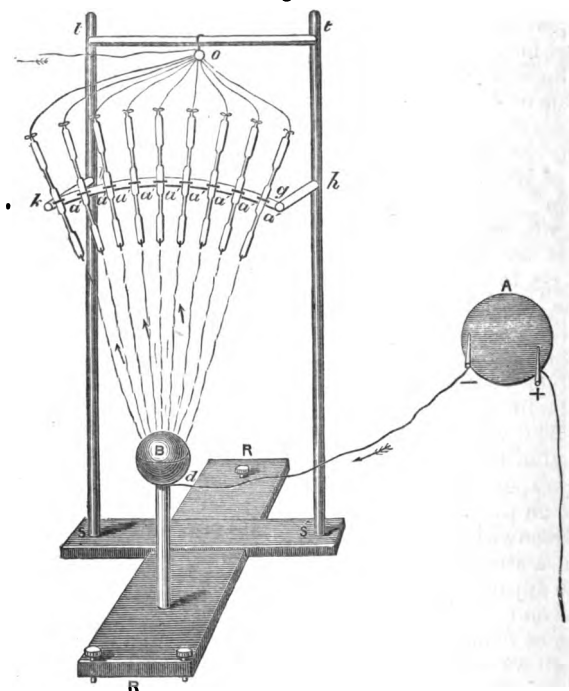
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'Daily Telegraph' newspaper, which on March 1, 1883, gave some additional particulars, and contained an interesting article *in extenso* on the subject, most of our scientific and literary journals were content to publish the telegrams as received, with but little added comment.

Now that the accounts are completed, it appears desirable to examine them more closely, taking the particulars, not from the original source, but from Prof. Lemström's further and more precise communications. The first idea of the use of such an apparatus appears to have occurred to the Professor as the result of observations made by Members of the Swedish Polar Expedition in 1868, on a variety of the auroral discharge taking the form of tiny flames, or a phosphorescent luminosity, during winter months in the polar regions, around projecting objects, notably mountain-cones and ridges. In order to call forth or magnify these phenomena, the Finnish Meteorological Society had an apparatus (similar

Fig. 1.



Prof. Lemström's apparatus, exhibiting the passage of electricity without light through air of normal pressure. A is the electrical machine; B an insulated conductor; a' , Geissler tubes connected by wires at one end, the other ends being free. The arrows and broken lines represent the non-luminous discharge and the direction of the current which lights up the vacuum-tubes.

to those employed later, but on a smaller scale) erected on Luosmavaara, a mountain-top about 520 metres high above Lake Enare; and on Nov. 22, 1871 (the same night it was erected) there appeared an Aurora, which began with a single perpendicular column of light above the top of Luosmavaara. This column, as examined by the spectroscope, was found to give the usual auroral citron line; and as at the same time other surrounding objects also did so, Prof. Lemström was led to believe he was "within a sphere of electric discharge whose radius extended considerably around the station." This was shortly after followed by Prof. Lemström's accidental discovery that a Geissler tube might be lighted up without any direct contact with either pole of the electric battery (our readers may recollect seeing the Professor's corona of tubes and electric machine exhibited at South Kensington illustrating this, *vide* p. 260), and to the deduction that an electric current "may traverse air of *ordinary* density without producing any light at all; but that when it encounters a layer of *very thin* air the luminous phenomena at once appear." In November 1882, it was determined to repeat and extend the mountain experiments; and after some preliminary ones on the summit of Orantunturi, 540 metres above sea-level, an "utströmnings" or "discharging" apparatus was erected thereon.

This is described as a long copper wire 2 millim. in diameter laid on insulators and fixed on poles $2\frac{1}{2}$ metres high, along which metal nibs pointing upwards were attached at each half-metre. The wire was arranged in the form of a rectangular helix, covering a surface-area of 900 metres, each inner coil being about 1.5 metre distant from the outer one. From the inner end a thinner insulated wire on poles 25 metres apart led to a hut at the foot of the mountain, and finally to a zinc disk in the earth, a galvanometer being interposed in the circuit. The insulators were constructed on the principle of M. Mascart, in which the glass stem is kept dry by an outer vessel containing sulphuric acid. The apparatus on Pictarintunturi was similar but smaller, and erected in two parts—the inner one covering 80 and the outer 320 square metres. From December 5, the day of fixing the apparatus on Orantunturi, there appeared "almost every night" a yellow-white luminosity around the summit of this mountain, "while no such luminosity was seen around any of the others." The flames were very variable in intensity, and in as constant oscillation "as those of a liquid fire." Tested $2\frac{1}{2}$ miles off by a small two-prism spectroscope, the light gave a "faintly continuous spectrum from D to F, in which the auroral line $\lambda=5569$ with soft variable intensity was observed." The galvanometer gave variable deflections, showing a positive current from the apparatus to the Earth. On December 29 there appeared above the apparatus on Pictarintunturi "a single column of aurora some 120 metres in height." Its plane was determined from several points, viz. S.S.W. to E.S.E., within 90° with the plane of the horizon; and there was "not the slightest doubt of the aurora appearing just above the apparatus."

The current as shown by the galvanometer was found to be in this case "proportionate to the surface-area of the apparatus laid out." Observations of comparison between the two mountains led to the conclusion that "the electric current from the atmosphere increases rapidly with the latitude" (Orantunturi lying in a higher latitude than Pictarintunturi, and nearer the plane of the aurora).

At Sodankylä the Aurora did not present many varieties. It began generally with a faint arc, developing into a sharp arc with streamers and luminous drapery. The "citron" line only was found by the spectroscope, and described as yellow-whitish with a slight shade of green. A four-prism spectroscope failed to give the line when the two-prism one did so.

Several observations are recorded in which the line was seen "without any aurora being visible." A yellow-white hazy phosphorescent luminosity, in which occasionally objects stood out clearly, is described as sometimes prevailing during the polar nights. The auroral line could not be seen in this, a fact which the Professor attributes to the too great dispersion of the spectroscope at his disposal. Some theodolite observations on auroral arcs are recorded, in which the distant observers were connected by telephone; and these tended to the conclusion that the two observers did not see the same aurora; and the Professor considers that "all measurements of the height of the aurora calculated on a long base north and south are always erroneous, as the observers never see the same aurora," adding "that the height of the aurora is very variable, I fully admit, but in my opinion it has been greatly over estimated." Researches with the terrestrial current are then treated of; and the Professor from these researches draws the inference that, "while the condition of the ground is of some influence, the terrestrial current ceases at a certain latitude." He also maintains that the terrestrial current "is caused chiefly by the electricity which descends from the atmosphere in the belt around the pole in which the aurora attains its maximum," pointing out that it seems probable the terrestrial current is closely related to the electricity in the auroral belt as well as to the magnetic variations, disturbances in Sodankylä of the terrestrial current being always followed by a magnetic one.

Finally, the Professor concludes "that the experiments at Luosmavaara in 1871, and at Orantunturi and Pictarintunturi in 1882, clearly and undeniably prove that the Aurora Borealis is an electric phenomenon."

On perusal of the above interesting and valuable contributions of Prof. Lemström to our stock of auroral knowledge, we cannot reasonably doubt that the apparatus on these occasions so ingeniously applied did really collect and make apparent to the eye a true auroral glow, its spectroscopic character being at the same time tested and defined by experienced observers. Yet one cannot help feeling something of regret that, if only for further assurance, the wave-length of the one line seen was not (as far as we are

aware) absolutely determined, on some occasions at least, and that the observations appear to rest only on a small instrument presumably without scale. That other lines of the auroral spectrum were not seen is perhaps not surprising, as they are frequently absent in auroræ of the yellow-white type, such as are common in Lapland; but here again it is possible they might have appeared had the spectrum been taken with a more specially appropriate instrument for such a purpose. In this regard, we would call attention to Mr. Backhouse's use (as described in his 'Spectrum of the Aurora' before referred to) of a prism or prisms without collimator or telescope, and in which a distant rather wide slit is viewed, the maximum of brilliancy for a faint spectrum being thus attained. There is also the possibility that the phosphorescent glow seen in Lapland by Prof. Lemström's party (and which has been also noticed in England, notably by Dr. Allnatt, the meteorologist, on September 24, 1870) might have yielded some spectroscopic result if analyzed by such a method. The oscillation of the auroral flames agrees well with the flickering of the aurora described by Professor Herschel, Mr. Backhouse, and others. The question of polarization of the beam or glow does not seem to have been made the subject of experiment (possibly for want of the necessary apparatus at hand), though it is one on which discordant observations have been heretofore recorded. The finding of the auroralines in parts of the sky where no aurora was visible is remarkable and noteworthy, though not without precedent, some early observers of the zodiacal light having been led to consider its spectrum identical with that of the Aurora probably from a similar cause, viz. a latent auroral glow. With regard to the height of the Aurora, Prof. Lemström's suggestion that it has been generally over-estimated may probably open a lively discussion, as undoubtedly his dictum will that "measurements of an aurora on a long base must be erroneous, as the observers never see the same aurora." Owing to the evidently complex arrangement of most auroral displays, with their array of shifting arches and beams, useful observations of portions of an aurora may be rare; but certainly some have been made (unless the observers have grossly deceived themselves) with a fair amount of success.

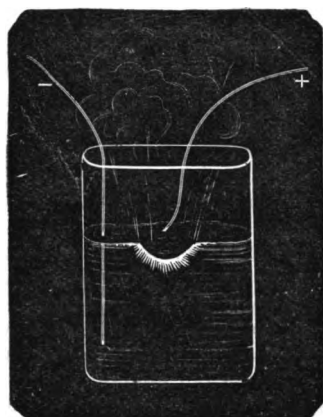
The Auroral beam of November 17, 1882, was considered to be the passing lighting up of an auroral arch, yet it had a sensible parallax, and its height was determined by two modes of calculation, with closely accordant results, at 133 English miles. Not to quote other authorities, Prof. A. S. Herschel has said, in speaking of auroral rayless milky arches, "I have got out the heights of these by good parallaxes of them, and they constantly occur within a few miles of 100 miles high." The observed connection of terrestrial and magnetic currents in Lapland may have been due to some local causes, for during some auroral storms (those, for instance, of November 1882 in England), while terrestrial currents were so strong as to interfere with State Telegraphic work, and to manifest

day auroræ by the vibratory movements of the Post-Office needles, atmospheric electricity was found comparatively quiescent.

The final and most important question of all is whether Prof. Lemström's experiments have "clearly and undeniably proved that the 'Aurora Borealis' is an electric phenomenon." In the sense that the auroral glow may be in some way and to some extent due to electricity this may be true; but in fact the nature of this electricity and of the other agencies (if any) involved in the matter seems left, notwithstanding the Professor's experiments, as mysterious as ever.

Until the birth of the spectroscope, savants and the public alike were content to receive the discharges in vacuum-tubes, and the brushes from electrified points, as true representations of the auroral discharge. Since that analytical instrument has come into practical use, it is recognized that the spectrum, and the spectrum only, is the true test of the identity of matter. Now how does the case stand in comparing the auroral spectrum with the air-spectrum, or rather the spectrum of the air-gases as excited by electric discharge? Assuredly no efforts have been left untried to tease the electric discharge into becoming an auroral one.

Fig. 2.



The above represents one of M. Planté's miniature Auroræ, "the arc and rays" formed between the poles of a powerful secondary battery.

Dr. De La Rue and M. Planté, with secondary batteries composed of elements innumerable, the late lamented President of the Royal Society, by ingeniously devised breaks applied to monster coils, Geissler and others, by tubes bearing their names, and Mr. Crookes, by his highly exhausted vacua, have each contributed to a simulation of auroral discharges and phenomena: and yet, when tried by the true touchstone, what has been the result?

In some instances the spectroscope has not been applied; but in all instances where it has, a failure to find an auroral spectrum

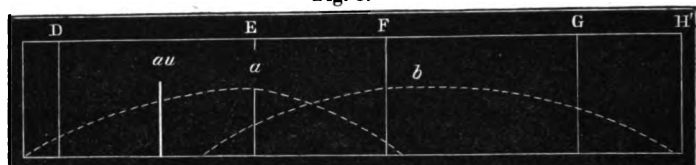
has resulted. Nothing fairly approaching in position or character to what Prof. Smyth has aptly called the "giant" citron line of the aurora spectrum, or its less conspicuous brother the red one, has ever been detected.

Attempts have been made by Dr. Vogel (to whom we owe the only accurately measured complete auroral spectrum) to identify the fainter lines of that spectrum with some of the lines of the air-spectrum; but Mr. Backhouse, the most recent authority on the subject, says (*Nature*, Vol. xxviii. p. 212), "They (the approximate coincidences) are sufficient to make the matter worthy of consideration, but perhaps this is as much as can yet be said." Prof. Angström in 1874 considered he had found an aurora spectrum in an exhausted dry-air flask; but the coincidences (which were mainly in the violet part of the spectrum) were by no means exact, and the characters of the spectra were not alike.

But the last-named Professor, while missing the actual spectrum, did make a valuable suggestion which acquires a renewed force from Prof. Lemström's recent discoveries. He suggested that the only probable explanation of the bright (auroral) line was "that it owed its origin to *fluorescence* or *phosphorescence*," adding "an electric discharge may easily be imagined which though in itself of feeble light may be rich in ultra-violet light, and therefore in a condition to cause a sufficiently strong 'fluorescence.'" Substitute in the last sentence the word "phosphorescence" for "fluorescence," and instead of ultra-violet light apply the observation to what Prof. Smyth calls the "citron" region of the spectrum, and we get valuable results from the Lapland experiments. First, they prove that the spectrum of the auroral beam of November 17, 1882, at a height of 133 miles, and that of an auroral glow in Lapland at the height of a few hundred metres, are identical, each consisting of the single citron auroral line accompanied by a faint yellow-green continuous spectrum, extending from about D to F (the principal region of phosphorescent light); and, secondly, if the experiments are fortunately continued, we shall no longer be troubled and perplexed by Dr. Vogel's "conditions of temperature and pressure perhaps not obtainable in our laboratories," for, setting aside the possibility of direct comparisons in the identical atmosphere, there cannot be much practical difficulty in reproducing such well-known conditions. Though the aurora spectrum is not an ordinary air-spectrum, it seems quite probable that the principal lines may be due to some form of matter excited by atmospheric electrical conditions; and some circumstances of the spectrum as well as of the visual character of the discharge (especially that seen in Lapland) strongly point to a phosphorescent agent in producing the two principal bright lines. Some of these are referred to in an article on tube-spectra compared with the auroral spectrum in the '*Philosophical Magazine*' for April 1875, ser. 4, vol. xlix. On plate vi. the spectra of the aurora and of a phosphoretted hydrogen-flame are compared, with some probability that if the broad bands of green and yellow light in the flame could be converted into lines

they might afford coincidences. M. Lecoq de Boisbaudran, in his 'Spectres lumineux' (texte, p. 188), has pointed out that the bands of the phosphoretted hydrogen spectrum under the influence of cold (when artificially *refroidie*) are intensified, one in the red becoming "from almost invisible, quite bright." See diagram below, in which the curves of phosphorescence and fluorescence are

Fig. 3.



In this diagram (of a normal spectrum), curve *a* is deduced from the spectrum of phosphoretted hydrogen, curve *b* from Prof. Angström's spectrum of the violet pole of air-vacuum tubes; *au* is the principal auroral line.

shown. Under the influence of cold the apex of the phosphorescence curve would move towards the auroral line, it being found by M. Lecoq de Boisbaudran that the less refrangible lines were more subject to this influence.

Science undoubtedly owes a heavy debt of gratitude to Prof. Lemström for affording us the opportunity of having, so to speak, an Aurora "at command;" and it will be our own fault if we do not by all accessible means avail ourselves of such a chance.

To do this there will be required not only zeal and skill, but money; and we are sorry to hear that it is possible these experiments may be discontinued, for want of resources to carry them on. We can hardly conceive in this temperate country the difficulties of fixing and maintaining the extensive apparatus required, and the wear and tear of these such a rigorous climate as Lapland involves. To this must be added (apart from the observers' personal expenses) the cost of subsidiary spectroscopic and chemical appliances. It can hardly be expected that all this can be met by a single local scientific society and its chief, however enthusiastic; and it is but just that assistance from the scientific world generally should be given in aid of such objects. The studies of sun-spots, earth-magnetism, and electricity have in most cases their material helps from public as well as private sources; and it will be hard indeed if their not less important relative the Aurora should be left out in the cold for want of these.

In these days of solving every thing, it is a positive luxury to find a mystery still waiting to be cleared up; and we ought to make an effort and not grudge a little money for the purpose of enabling the line of research which Prof. Lemström has so happily inaugurated to be followed up.

J. RAND CAPRON.

Guildown, 1883, August 15.

P.S.—I am glad to find, since the above was penned, that the Swedish Diet has voted £1500 in aid of these objects.—J. R. C.

Spectra of two hundred and fourteen Flashes of Lightning, observed at the Astrophysical Observatory, Herény, Hungary.

I HAD just paid a visit to my friend M. de Gothard on July 22, 1883, when a heavy thunderstorm arose in the west, in the direction of Stiermark. M. de Gothard having agreed to my proposal to attempt to secure some spectroscopic observations of the lightning, we watched the storm, till wind and rain prevented a longer stay, from the dome of the 10" reflector, and observed the spectra of 186 flashes. We then continued our observations in the physical saloon, looking out east and southward, following the course of the thunderstorm. M. de Gothard firmly mounted a spectroscope towards the point where the frequency of the flashes attained its maximum, and whilst he indicated the character of the spectrum, I noted the characteristics of the lightning itself, such as glare, horizontal zigzag, flash. We were so fortunate as to obtain 28 simultaneous observations in this manner.

During the earlier observations, we used a little star-spectroscope with a Merz prism belonging to M. de Gothard, and a similar direct-vision spectroscope by Dr. Hugo Schröder with slit but without telescope, which belonged to me. Each instrument had a scale. The simultaneous observations were made only with the former instrument, because it had a permanent lamp for illuminating the scale.

The result of the earlier observations is as follows:—

99 of the 186 flashes showed the nitrogen spectrum, 28 the bands of nitrogen, together with 3 exceedingly bright hydrogen bands; 23 flashes had only the 3 bright hydrogen bands, and 36 had a continuous spectrum.

At the beginning of the thunderstorm we mostly observed continuous spectra, or the nitrogen bands; later the hydrogen bands appeared more and more frequently.

In the simultaneous observations we saw 19 horizontal zigzags, 6 aerial glares, and 3 flashes. 8 of the first species showed but one preponderating bright line, 6 showed two bands, and only one had three very bright ones; scale-readings 5, 10, 15. Two evidently showed bands of nitrogen, one showed bands of hydrogen, and one had an exceedingly large number of lines.

Five of the glares were seen with one line (scale-reading 10), and one with a continuous spectrum.

Flashes are rare apparitions, so I regard it as a most fortunate occurrence that we should have observed three with the spectroscope on one evening.

The first had only one remarkable bright band, another two bands (scale-readings 5, 15), and the third a nitrogen spectrum of abnormal intensity, showing with some certainty the absorption bands of atmospheric vapour.

After the thunderstorm had decreased, we made a few observations for determining the scale of the spectroscope. Though the

apparatus was not yet quite finished, we used it on account of its convenience of management and illumination.

As mentioned, M. de Gothard observed the brightest bands at scale readings 5, 10, and 15, and once a line at 20 and another at 26.

In the apparatus, the brightest nitrogen band was at 3, the second at 10.5; the hydrogen bands lay at 4 (C) and 16.5 (F).

The more intense bands of carbon dioxide were visible at 5.0, 8.5, and 13.0.

According to our observations, the bands in the spectra of flashes might coincide with any of the above-mentioned ones. The cause of this uncertainty in these observations will be best appreciated by those who have made such observations themselves. I would mention that we did not see any bright sodium line at all, though we looked specially for it.

The observations were made from 10^h 15^m till 11^h 10^m, M. Herény time, and I may say that only half of the flashes were examined.

O'Gyalla Observatory,
Hungary.

NICOLAS DE KONKOLY.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Liverpool Astronomical Society.

SIR,—

In affairs astronomical it is too often considered that the Royal Astronomical Society supplies all existing requirements. As well might it be said that Universities render all other scholastic institutions a superfluity. It is partly to correct such an impression, and partly to encourage provincial societies of a like nature, that I venture to submit an outline of the founding and working of the Liverpool Astronomical Society.

This Society originated at the close of an Astronomical Lecture, when it was proposed by three or four of the audience that a study of the science would be facilitated if amateurs would meet at stated intervals and compare the results of their observations. The suggestion was well received, and, for a short time, acted upon but a proselytizing spirit grew upon the promoters, and it was determined to make an attempt to found a popular society, whose object should be to foster a liking for astronomy, especially amongst the possessors of small telescopes. The latter clause, however, introduced a difficulty we had not foreseen; for those most wishful to join did not possess a telescope at all; whilst their only experience of observational astronomy was such as might be obtained any fine night (particularly at full moon) for a penny, at the street corner.

We had therefore not only to "foster a liking" for observation, but to provide the means as well. This was accomplished by laying in a stock of 2-inch object-glasses, and some lengths of brass tubing, which were speedily disposed of and ultimately developed into telescopes of more or less accuracy, under the hands of the owners. We were now a Society of ten members, fully equipped for work, and as such considered ourselves justified in soliciting the countenance and literary assistance of local Astronomers, which in most cases was promptly and favourably responded to. Appeals were also made to Fellows of the R. A. S. and other active observers throughout the country, for papers on subjects connected with the science. In this also I do not think we met with a single refusal, although the contributions were not infrequently accompanied by warnings that similar experiments had been tried in other towns, and that failure was rather a question of time than of fact.

At the end of the first session we began to realize that these gloomy forebodings were not without foundation, for we numbered but 17 members all told, of whom, now that the novelty had worn off, not one half could be reckoned as active workers. Astronomers, however, and particularly amateurs, are not easily discouraged; and although our hopes were a little dashed, we felt more than ever convinced that a kind of halfway resting-place between the amateur public and the Royal Astronomical Society was a necessity that must, before long, be appreciated. At the close of the second session, I am now happy to state that our confidence has been fully justified by the result, for we number more than 70 members, and the Society is self-supporting.

It would, however, be ungrateful to claim that this position has been due to our own efforts alone; and I should here like, on behalf of the Society, to acknowledge the kindly assistance we have invariably received from observers both in England and America, but especially for that given at a time when it appeared as if the only recompense we could offer was that they should bear the discredit of our failure rather than share in our success.

Yours obediently,

W. H. DAVIES,
Sec. L. A. S.

Tempel's Periodical Comets.

SIR,—

M. Tempel has been such a successful discoverer of comets, several of which have turned out to be not only periodical, but with periods of nearly equal length, that there has sometimes been a little confusion in distinguishing between them. It has occurred to me, therefore, that your readers may be glad of a list of those three the orbits of which are of that character, especially as one of them is expected shortly to make its appearance again.

The first (long known simply as Tempel's Periodical Comet) was discovered on the 3rd of April, 1867, and passed its perihelion that year on the 24th of May. The period is very nearly six years, and

the comet was observed again in 1873 and 1879, passing its perihelion in the latter year on the 7th of May. The next return will be due in the spring of 1885.

The second of these periodical comets was discovered by M. Tempel on the 3rd of July 1873, after it had passed its perihelion on the 25th of the previous month. Its period is about five years and a quarter, and it was duly observed again in 1878, passing its perihelion on the 7th of September. This is the comet which will soon be searched for again, another return to perihelion being due in the autumn of the present year. It will be noticed that, in the year of its discovery, this comet passed its perihelion only about six weeks after the former, the two passages occurring in 1873, on the 10th of May and the 25th of June respectively.

The third comet was discovered by M. Tempel on the 27th of November, 1869; but its periodicity was not recognized until after its rediscovery by Prof. Swift at Rochester, N. Y., on the 10th of October, 1880. The period is about five years and a half, so that an unobserved return must have taken place in the spring of 1875. The perihelion passage in 1880 took place on the 8th of November; another will be due in 1886, but, owing to the comet's unfavourable position at that return, it is not likely that it will be seen until the subsequent one in 1891.

Yours faithfully,

W. T. LYNN.

Blackheath, 1883, August 10.

Large Meteors.

SIR,—

I venture to forward you an account of an unprecedentedly large meteor which showed itself here last evening, and beg to accompany it with a sketch plan* giving details of position &c., which may be of some interest to you.

I happened to be lying on my charpoy (a portable native bedstead) in the open, star-gazing, and was occupied with Dubhe (α Ursæ Majoris), when suddenly, to my great astonishment, what seemed like a star of the 3rd magnitude showed itself a degree or two to the left of α Ursæ and in the same plane. Simultaneously it began to brighten to first magnitude, and then moved in a perfectly vertical line, first at a slow pace, but increasing to a terrific rush as it neared the horizon. Its colour when I caught sight of it was red (something like Antares); this changed, directly it began its flight, to orange, yellow, next white, and as it fell into the sea it seemed an intensely vivid steel-blue, like Vega.

The tip of the tail never left the spot from whence the nucleus or head emanated; and as it shot down with its immense tail it greatly resembled our late 1882 Comet, except that its brilliancy and size of nucleus was infinitely greater. From a mere point at $\circ$ the head increased in size until within 5° or so of the horizon,

* [This sketch plan has not been reproduced, as it seemed not wholly consistent with the description.—ED].

when it appeared as large in diameter as the setting sun. The moment of impact with the horizon showed it in all its glory. Its brilliancy at this moment was such that I had to avert my eyes; it was impossible to gaze directly at this majestic ball of fire, which looked only a few hundred yards off. The whole of the N. and N.W. region of the heavens was lighted up so much as to blot out all the stars in that quarter; the country around our Telegraph Station and the hills of Gāigun in the distance, 20 miles away, became distinctly visible—in fact, to use a more lucid expression, “night became day.”

Such a stupendous object as this had never been seen by the oldest Beluch inhabitant, and naturally the villagers beheld it with consternation; even I, with the full knowledge of its innocent character, could not help a feeling of awe and amazement overcoming me. The nucleus appeared to fall into the sea 60° west of true N., between ourselves and the mainland opposite 10 or 12 miles off. Until the disappearance of the head its tail was fan-shaped; but directly it vanished both the top and bottom portions of the tail did the same, leaving only a thin whip-like streak of

Jask Telegraph-station, Persian Gulf, lat. $25^\circ 38' 19''$ N., long. $57^\circ 45' 42''$ E.
 Meteor entered our atmosphere at 7.51 P.M. Luminescence in sky
 lasted from 7.52 to 8.33 P.M.

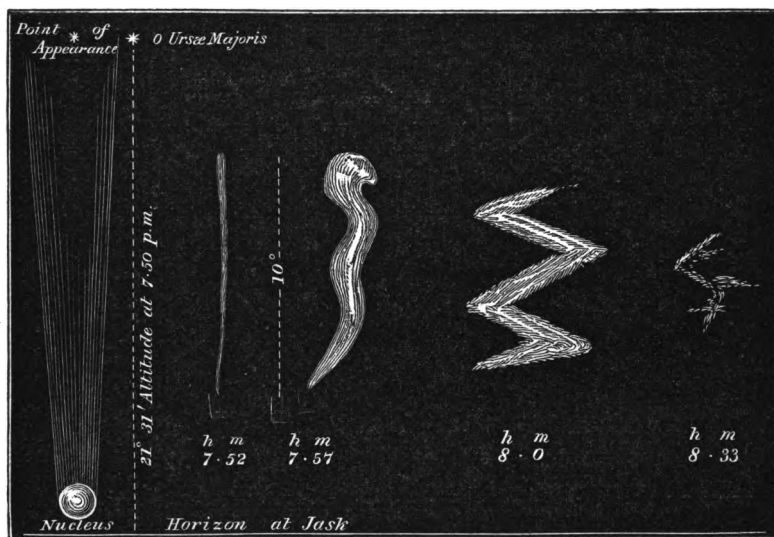


Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

flame (probably meteoric dust) midway between *o* and the horizon. This streak was quite 10° in length, the altitude of *o* from its top being $21^\circ 31'$. This was at 7.52 P.M.; a moment afterwards we

noticed it beginning to widen and assume a serpentine form (something like an eel with a distorted head); but as we watched it we were surprised to find it becoming rapidly *radiant with light*! This luminosity attained its maximum (that of an opal-globed lamp) in 5 minutes, remained constant for another 12, and then gradually faded down. At 8 P.M. the tadpole or eel-shape had been converted into the zigzag one shown in the enclosed figure. This was possibly due to currents of air traversing and breaking up its continuity. In another half-hour all that was left to mark its position was a small stratus-like cloud, and this too disappeared at 8.33 P.M.

I may add that this meteor, unlike some others, did not burst or scatter, but passed, as it were, into the sea with an ever-increasing brightness. I am now endeavouring to ascertain from the natives about whether they noticed it, as I did, to fall between ourselves and the opposite mainland; and if I can I will also find out from the people about the Gāigun hills, if it passed between them and our station; in such case the locality of its actual disappearance must be confined to the bay, which at this spot is only 10 miles across.

I am, Sir, yours very obediently,

Cape Jask, Persian Gulf,
1883, June 9.

H. HARRISON,
Persian Gulf Telegraphs.

SIR,—

A large meteor was seen here on August 3. Brightness = 4 or 5 times Jupiter. Colour reddish. Diameter = $\frac{1}{8}$ Moon's. Path from the Pleiades to μ Arietis. Duration of flight = 5 seconds. Motion very slow. It did not burst but died gradually out, leaving a faint train of sparks. Time of observation 13^h 11^m, G.M.T.

Coats Observatory, Paisley,
1883, Aug. 7.

Yours faithfully,
DONALD MACLEAN,
Observer.

The Red Spot on Jupiter.

SIR,—

When, on the morning of August 24, at 4^h 30^m, I turned my 10-inch reflector on Jupiter, it was with considerable doubt as to the continued visibility of the red spot. A first glance at the planet seemed to prove that the spot had entirely disappeared, although the curious hollow in the great southern equatorial belt, immediately contiguous to the spot, was very plain, and the details of the belts came out with great distinctness. After some attention, however, I noticed a very faint shading lying S. of the hollow referred to, and this zone of the planet was perceptibly brighter to the E. and W. of the position formerly occupied by the red spot. This notable marking is therefore still visible, though of the last degree of faintness; it came to the central meridian of Jupiter at 16^h 48^m, August 23, and I believe the time is fairly accurate. The spot now follows Mr. Marth's assumed first meri-

dian by $1^h 51^m$; and if the slackening motion of this object continues at the same rate as during the past two years, then this interval will increase by about 4 minutes per month. The spot being at present an object of extreme delicacy and apparently on the verge of utter obliteration, will enable but few more observations to be made; but it is to be hoped that the conspicuous hollow in the S. side of the great southern belt will continue to be watched with especial care. This feature is an important one, as it shows the same rotation-period as the red spot, and affords an excellent clue to the exact position of that object.

Yours faithfully,

Bristol, 1883, Aug. 24.

W. F. DENNING.

P.S.—Aug. 25. This morning I observed a group of three white spots near the equator of Jupiter. The preceding spot crossed the C.M. at 4.12 A.M., the central one at 4.35 A.M., and the following one at about 4.45 A.M. From the appearance and position of the central spot there can be no doubt that it is identical with the well-known equatoreal white spot which has attracted so much attention during the last few years.

W. F. D.

SIR,—

The following observations of this object on Jupiter's central meridian have been made here with a power of 150 on a $5\frac{1}{4}$ -inch Calver reflector:—

Preceding end .. 1883, Aug. 23, $16^h 23^m$, G.M.T.

Centre „ „ $16 47$ „ „

The observation of the centre is probably a little too early, owing to the increasing daylight and badness of definition. About 10^m earlier, occasionally for an instant, not only could the outline of the spot be distinctly seen of its usual form, but also a decided pale reddish tinge. It is, however, an exceedingly faint object, and but for the great hollow in the double south equatoreal belt, a very difficult one to see at all. More usually it appeared as a shapeless mass of slightly warmer colour.

The red spot therefore now follows Mr. Marth's first meridian by an interval of about $1^h 50^m$, so that its retrograde motion has been maintained. The other features of the planet, apparently remain in general the same as in last spring.

Yours faithfully,

West Brighton, 1883, Aug. 24.

A. STANLEY WILLIAMS.

An old Sun-spot Observation.

SIR,—

A short time ago, as I was turning over the leaves of a copy of Boyle's works*, I met with the subjoined. It is probably

* The Works of the Honourable Robert Boyle. London: printed for A. Millar, opposite Catherine Street, in the Strand, MDCCXLIV., Vol. iii. p. 213.

known to you and your readers. If so, of course you will suppress this ; if not, it may interest some, as it interested me, who had lately been pondering on the scantiness of the recognition which had been given to the meteorological work of the late Sir E. Sabine in reference to sun-spots. The most noteworthy part is perhaps the final paragraph containing the suggestion of Mr. Hook. I carefully reproduce the punctuation.

Your obedient Servant,

F. GUTHRIE.

S. Kensington, 1883, Aug. 11.

An Observation of a Spot in the Sun. First printed in the 'Philosophical Transactions, No. 74, p. 2216, for April 27th, 1671.

"Friday, April 27, 1660.

"About 8 of the clock in the Morning, there appear'd a Spot in the lower limb of the Sun, a little towards the South of its *Æquator*, which was entred about $\frac{1}{10}$ of the Diameter of the Sun, itself being about $\frac{1}{10}$, in its shortest Diameter, of that of the Sun ; its longest, about $\frac{1}{10}$ of the same. It disappear'd upon Wednesday Morning (May 9th) though we saw it the day before, about 10 in the morning, to be near about the same distance from the West-ward limb a little South of its *æquator*, that it first appear'd to be from the East-ward-limb, a little South also of its *æquator*. It seem'd to move faster in the middle of the Sun than towards the limb. It was a very dark spot almost of a quadrangular form, and was enclosed round with a kind of duskish cloud, much in this form and in this proportion to the spot.



"We first observ'd this very same Spot, both for figure, colour and bulk, to be reentred the sun, May 25th, when it seem'd to be in a part of the same line it had formerly traced ; and was entered about $\frac{1}{3}$ of its Diameter about 7 of the clock in the afternoon *. At the same time there appear'd another Spot, which was just entred, and appear'd to be entred, not above $\frac{1}{10}$ part of the Sun's diameter. It appear'd to be longest towards the North and South, and shortest towards the East and West. There seem'd to be dispers'd about it divers small clouds here and there."

[These observations were made, as the Noble Observer told us, with an excellent Telescope, in the presence of divers Curious and Ingenious persons, one of whom was Mr. *Hook*. And discoursing of thoughts he had entertained touching the Effects of such Spots, he suggested this inquiry, Whether they might not cause a considerable alteration both in the Body of the Sun itself and in our Air, and the Bodies in it upon their dissipation ?]

* These rough data appear to indicate a period of solar revolution of about 24½ days, supposing polar and equatoreal rotation to have the same angular velocity.—F. G.

NOTES.

URANUS*.—Prof. Safarik, of whose paper on the ellipticity of Uranus we published a translation in the June number of the 'Observatory,' called Prof. Schiaparelli's attention as early as 1881 to the favourable opportunity now presented for examining the figure of the planet. Prof. Schiaparelli was not, however, able to undertake the work until the spring of the present year. The planet seemed to him very distinctly elliptical at the first examination, but the actual measurement of the difference between the two diameters was somewhat difficult. The beautiful telescope of 8 inches aperture of the Brera Observatory showed the planet exceedingly well defined on every favourable occasion; but Prof. Schiaparelli had several times to utilize nights when the limb of the planet was undulating and ill-defined. The powers ordinarily used were those of 417 and 500, less frequently 322 and 690, and the measures were made between April 12 and June 7.

The measurement of the position-angle of the axis was made by placing one of the wires on the planet in such a manner that the two halves appeared symmetrical. Twenty-three observations made on twenty-two nights gave a mean value of $197^{\circ}.3$ for the position-angle of the major axis of the apparent disk on 1883 May 6. The position-angle of the major axis of the apparent orbits of the satellites was $195^{\circ}.4$ on 1883 April 12, and $195^{\circ}.3$ on 1883 June 7. The difference of 2° is much less than might reasonably be expected from so difficult and uncertain an observation. It therefore seems highly probable that the orbits of the satellites of Uranus, like those of Mars, Jupiter, and several of those of Saturn, lie in the same plane as the equator of their primary. Twenty-five determinations of the ellipticity were obtained on sixteen nights, each determination resulting from two measures apiece of each diameter. The result gave for the equatorial diameter A, at the distance 19.1826, $3''.911 \pm 0''.020$, the probable error of a single measure being $\pm 0''.099$. The value of the minor axis, B, was $3''.555 \pm 0''.024$, probable error of a single measure $\pm 0''.119$. The

resulting ellipticity will be $\frac{1}{10.98 \pm 0.93}$, or nearly the same as that of Saturn; or, calculating the ratio $\frac{B}{A}$ for each evening separately,

$\frac{1}{10.94 \pm 0.67}$, a result closely according with Mädler's value of $\frac{1}{10.85}$. Prof. Schiaparelli, to test his measures, drew the outline of the planet on thirteen nights, and measured the drawings, obtaining $\frac{1}{11.07}$ as the value of the ellipticity. Sig. Fornioni, an assistant

* Astron. Nachr. Nos. 2526 and 2528.

of the observatory, obtained $\frac{1}{10.9}$ in the same way on May 21 and 194°9 for the position-angle of the axis.

Dr. E. Millosevich also observed the planet with the 9-inch equatoreal of the Royal Observatory at Rome during the months of April and May, but without detecting any decided ellipticity; but nearly all his measures were made with a position-angle of 0°. The measures give the semidiameter of Uranus 3".633 at the distance 19.1826. Sig. Millosevich intends to publish a memoir shortly, in which the details of all the measures which have been made of the diameter of the planet from Herschel's to Schiaparelli's will be given.

Prof. Schiaparelli whilst making the above-mentioned measures was able to ascertain that there are spots and differences of colours visible on the disk of Uranus, and his assistant confirmed the observation. But after making many fruitless attempts to sketch the spots, or at least to ascertain directly the rotation of the planet, he was led to the conclusion that an aperture of 8 inches was insufficient for the purpose, though with good definition he thought it would not be beyond the powers of some of our great modern telescopes.

THE DIVISIONS IN SATURN'S RINGS*.—Prof. Kirkwood showed some twenty years ago that Jupiter exercised a peculiar influence over the minor planets, tending to produce well-marked gaps amongst them at certain well-defined distances. For if the period of any minor planet were commensurable with that of Jupiter, the latter would exercise a perturbing influence upon it, which would eventually result in a complete change of orbit. Later on in 1868, Prof. Kirkwood employed the same principle to account for the great division (Cassini's) in Saturn's rings. Maxwell had shown that the rings must be formed of separate particles moving round the planet to a certain extent as independent satellites. But a body moving round Saturn at the distance of Cassini's division would have a period that was very closely commensurable with those of each of the six inner satellites, and it would therefore be especially exposed to perturbation. Dr. Meyer has carried the principle yet further, and has investigated every possible combination of the commensurabilities of the revolution periods of the satellites, and he finds that, including the division of Cassini, there are seven places where the satellites would unite to exercise a perturbing influence on the members of the ring system. The first position is where the period would be one fourth of that of Mimas, and marks the inner boundary of the dark ring. Particles moving at almost precisely the same distances would have their times commensurable with each of the other five inner satellites; thus,

* Astron. Nachr. No. 2527.

for a period of $\frac{1}{4}$ of that of Mimas we have a distance of $10''.56$ from the centre of Saturn, for $\frac{1}{8}$ of that of Enceladus $10''.43$, and for $\frac{1}{8}$ of that of Tethys $10''.66$. Dr. Meyer sees a consequence of this close agreement in the well-defined character of the inner edge of the dark ring. Next comes Struve's division in the dark ring. One fifth the period of Enceladus corresponds to a distance of $11''.79$, one seventh that of Tethys $11''.66$; the three next satellites give a closely similar result. The position of Struve's division is not very exactly known, and Dr. Meyer adopts $11''.79$ * as its distance, being the mean between the positions of the inner boundaries of rings C and B. One third of the period of Mimas introduces a new series of commensurabilities in which all the six satellites take part, but the agreement is by no means so close as in the first two cases; and Dr. Meyer regards the indistinct character of the inner boundary of the bright ring B, which would about correspond to the mean of the distances indicated, as connected with this less perfect coincidence. The period of Enceladus is 4 times, that of Tethys 6 times, that belonging to a particle at this distance. Cassini's division corresponds, as already stated, to a period commensurable with each of the six inner satellites, the period of Mimas being twice as long, Enceladus 3 times, Tethys 4, Dione 6, Rhea 9, Titan 33. The commensurabilities in the case of the four nearest satellites are of the simplest possible character; and we find that the inner edge of Cassini's division, which is situated at the distance thus indicated, is especially distinctly marked. The outer edge is very indistinct, the influence of Rhea and Titan being much feebler on account of their greater distance.

One fifth the period of Dione corresponds to about the distance of Encke's division. One eighth of Rhea's period and one half of Titan's approximate roughly to the same distance. The division is faint and ill-defined. One third the period of Tethys, the simplest relation now remaining, indicates the outer boundary of the ring system, and $\frac{1}{4}$ that of Rhea, and $\frac{1}{8}$ that of Titan correspond to distances of nearly the same amount.

The only simple relation omitted is that of $\frac{1}{8}$ the period of Tethys, and this closely corresponds to integral parts of the periods of the three next outer planets. There should therefore be another division at about $14''.7$. Dr. Meyer does not seem aware of the fact; but several observers of Saturn have noticed that ring B begins to shade off a little nearer Saturn than the centre of the ring, which would correspond to a distance of about $14''.7$ or $14''.8$. Prof. Holden speaks of the point where this shading off begins as "a definite point." The correspondence between calculation and observation as to the divisions of Saturn's rings would therefore seem to be complete.

* There is a misprint here in Dr. Meyer's paper; the observed and calculated distances have been interchanged.

THE ORBITS OF THE SATELLITES OF SATURN.—On 1883, July 5, Dr. W. Meyer communicated a paper to the Société de Physique et d'Histoire naturelle de Genève, on the results of his observations of Saturn made during the year 1881, an abstract in fact of an extended memoir shortly to be published on the same subject. The substance of the early part of the paper appeared in the article translated from the 'Astronomische Nachrichten' No. 2517, in the 'Observatory' for July. An abstract of a second portion is given in the preceding note; and the remainder of the paper is occupied with the results of Dr. Meyer's calculations of the orbits of the Satellites, and of the mass of Saturn. Dr. Meyer succeeded in observing all the satellites except Hyperion, which, however, he saw on one occasion. Mimas was observed five times, and seen on three other occasions. Enceladus was observed on 30 occasions; Iapetus only on 16, it having been lost during its eastern elongation, from 1881, Sept. 29, to Oct. 26. In the present instance Dr. Meyer has abandoned the approximative method he employed in the calculation of the orbits of four of the satellites from the 1880 observations, and followed the more laborious but more precise method employed by Bessel in his calculation of the orbit of Titan. He first calculated the apparent positions of the satellites for the moments of observation from approximate orbits, and then, comparing the observed with the calculated places, deduced what he terms definitive orbits for the six satellites. The following are the mean distances (for distance 9'5389), and periods thus obtained:—

Mean distance.		Period		
		d	h	s
Enceladus	34'35.01	1	8	53.692
Tethys	42'75.14	1	21	18.2562
Dione	54'75.74	2	17	41.929
Rhea	76'48.38	4	12	25.1157
Titan	176'9.102	15	22	41.2316
Iapetus	514'7.108	79.	7	49.2484

Comparing Bessel's orbit of Titan with his own, Dr. Meyer finds the following differences:—

	M—B
Mean longitude, 1881, Nov 0.0	= $L_0 = +1^{\circ} 13' 12''.0$
Mean distance in equatoreal radii of	
Saturn	= $a = +0''.3581$
Eccentricity	= $e = +0.000646$
Longitude of perisaturnium	= $\pi = -15'.16''$
Longitude of node on the ecliptic	= $n = -23''.1$
Inclination of orbit to the ecliptic	= $i = +5'15''.1$

The smallness of the differences prove Bessel's elements to have been exceedingly correct. The difference of $1^{\circ} 13' 12''.0$ in the mean longitude has accumulated during 1190 revolutions of the satellite, and agrees closely with that observed in 1880 by Prof. Asaph Hall, which for 1880, December, was $+1^{\circ} 10'.3$. The result

ing correction to the period is 4". The difference in the distance arises from the fact that Bessel always measured distances with the Königsberg heliometer as smaller than those measured by observers with filar micrometers. The secular variations of the positions of the perisaturnium and of the node may be considered as very well determined. The comparison of the two orbits gives for these tropical movements in a Julian year,

$$d\pi = +1811''\cdot5 \quad dn = +35''\cdot084$$

A comparison of Dr. Meyer's orbit of Iapetus with that derived by M. Tisserand from the Washington observations in 1874 gives the following differences.

$$\begin{array}{l} \text{M-T} \\ a = +0''\cdot341 \\ e = -0\cdot00065 \\ \pi = +5^{\circ} 5'\cdot1 \\ n = +1'\cdot5 \\ i = -4'\cdot7 \end{array}$$

The tropical secular variations of the perisaturnium and node would be

$$d\pi = +2514'' \quad dn = -17''\cdot31$$

THE MASS OF SATURN.—Dr. Meyer has also calculated the mass of Saturn from each one of the Satellites, making due allowance for the influence of the Sun, the rings, and the polar flattening of Saturn, the last element having been omitted by Bessel in his computation of Saturn's mass from the movement of Titan. The results are as follows:—

	Mass.	Weight.
Enceladus	0·000284213	1·0
Tethys	0·000288215	4·4
Dione	0·000288531	9·1
Rhea	0·000288898	15·5
Titan	0·000287204	73·1
Iapetus	0·000285849	48·6

The mean mass obtained by giving the above weights to the individual values being

$$M = 0\cdot0002871145 \text{ or } \frac{1}{3482\cdot93 \pm 5\cdot50}$$

The above value includes the rings. Dr. Meyer finds the reciprocal of their mass in terms of that of Saturn as $m = 119\cdot1$, Bessel's value being $m = 118$.

UNPUBLISHED STAR-GAUGES OF SIR WILLIAM HERSCHEL *.—Beside the 683 star-gauges published in the 'Philosophical Transactions' for 1785 (p. 221), Sir William Herschel made 400 more in various parts of the heavens which have not yet been published. These

* Science, vol. ii. No. 25.

have lately been extracted from the original observing-books preserved at the Herschel family residence at Collingwood, through the kindness of Sir William Herschel, the present baronet, and of his brother, Major John Herschel; and the manuscript has been presented to Prof. Holden, director of the Washburn Observatory. The original records are in the handwriting of Caroline Herschel, and by her care every detail necessary to their accurate reduction is preserved.

SUNSPOTTERY*.—The design of this little pamphlet is to give an impartial account of the results of the investigations into the connection between solar activity as evidenced by the number of sunspots, and various terrestrial phenomena and events. That the author believes himself to be impartial may be conceded; that he is entirely so may be doubted from the very title he has selected, seeing it has hitherto only been employed in an offensive sense. And the doubt is intensified when we find him asserting that sunspots do not vary in number periodically—an assertion which though perhaps justifiable when taken in one particular sense, can hardly fail to lead an uninstructed reader astray. He does not deny the marvellous connection between sunspots and terrestrial magnetism; but strives by a skilful reference to the supposed “lagging” of the magnetic curve, to convey the impression that the connection is less intimate than is really the case. He does not give full prominence to the natural argument that so strong and intimate connection between solar and terrestrial phenomena in one respect infers connections, it may be far less marked, but real nevertheless, in others. Yet this has been precisely the reason why all the various inquiries which he reviews have been undertaken; and whether they have proved successful or not, was abundant justification for their institution. And if they have been less fruitful than their authors had hoped, Mr. Oliver at least should be tender to them; he is as ready, on slender proof, to detect “influence” on the weather as any one, but looks to the moon, not to the sun, as the influencing body; and this, despite Mr. Ellis’s investigation into the subject (*Phil. Trans.* vol. xxxiv. p. 61), to which he does not even allude. Still, since now-a-days popular writers are more prone to outrun scientific discovery than to lag behind it, Mr. Oliver’s pamphlet, which is certainly ably written from his point of view, may prove a wholesome corrective, and may, we will hope, diminish the influence of some of the sensational writing of which we often have too much.

WASHBURN OBSERVATORY†.—Prof. Holden during the year 1882 used the 15½-inch Alvan Clark equatoreal in the regular observation

* Sunspottery: or What do we owe to the Sun? A popular examination of the Cycle Theory of the Weather, Famines, Pestilences, Commercial Panics, etc. By J. A. Westwood Oliver. London: Simpkin, Marshall and Co. 1883. 56 pp.

† *Vierteljahrsschrift der Astr. Gesell.* 1883, Heft 2.

of sweeps after Herschel's method, and found 95 new doubles and 4 new nebulae, besides making notes on a large number of red or coloured stars. The 6-inch equatoreal, formerly belonging to Mr. S. W. Burnham, has been used mainly for purposes of instruction; but 21 observations of occultations were secured from 1882 October to 1883 January for comparison with the work of the transit-of-Venus parties. The first two contacts of the transit were secured by two independent observers. Prof. Holden has undertaken the reduction of the 38,000 observations of zone stars made at Washington in the years 1846 to 1849. In connexion with this work Prof. Holden remarks:—"A very simple device, borrowed from the card-catalogue system of librarians, has saved me so much trouble that, small as it is, it may be worth mentioning. I copy each observation on a separate card 6.5 × 4.5 inches. All the reductions are put on this card, and the final positions are written in red ink. Notes go on the back of the card. The last step of the work will be to arrange these cards in the order of R.A., to number them, and to send them to the printer. He prints only what is written in red ink. In this way much copying is saved." During the months of June and July Prof. Holden tested the efficiency of Prof. Watson's Solar Observatory built for the detection of Vulcan near the Sun, and consisting of a device whereby a dew-cap 55 feet long was applied to the 6-inch refractor formerly belonging to Mr. Burnham. The dew-cap pointed to the pole, and the Sun's rays were reflected down by means of a heliostat. Very little advantage, however, resulted from the arrangement, and it was found to be impossible to detect by daylight even so bright a star as η Tauri when 50° distant from the Sun, conclusive proof that the device would have been useless in a search for Vulcan.

THE LICK OBSERVATORY*.—Much progress has been made at the Lick Observatory during the past year. The dome for the 12-inch equatoreal has been entirely completed in a very thorough manner. It is asserted to be the most convenient and complete dome of the size in the United States. The 4-inch transit-house, and the buildings for the photo-heliograph, are in capital working order; they were utilized last December in a very successful observation of the transit of Venus. The walls of the main building are half done, and the cellar for the dome of the 36-inch equatoreal is excavated. Many of the original arrangements of the buildings and grounds were only provisional, and these are being replaced by others more substantial and permanent. A brick reservoir containing 83,000 gallons of water (derived from three springs) has been built during the season, another of 20,000 gallons (spring-water), and another of 83,000 gallons (rain-water) will shortly be begun. The roads have been extended. The house for the meridian circle (Repsold) will be begun in a few weeks, as well as a house for the astronomers, and buildings to contain the

* 'Science,' Vol. ii. No. 22.

appliances for heating and lighting the buildings and moving the dome. The end of this season will show great progress.

RECENT SOLAR DISTURBANCES *.—M. Thollon has recently observed some remarkable solar disturbances. On July 22, the southern hemisphere of the Sun presented a very unusual appearance, a long chain of spots at nearly regular intervals stretching from limb to limb in the southern hemisphere. Near the east limb was a large group formed by a great number of little spots, too many to be counted. The approach of this group had been signalized on July 16 by a small but very brilliant prominence. At 4 o'clock in the afternoon, the prominence consisted of a number of straight jets apparently diverging from the same point of the limb. On this prominence M. Thollon saw some most pronounced displacements of the C line, one of them, observed at 5^h 20^m, indicating a motion of approach of about 300 km. per second. An hour earlier, in the same region, a smaller displacement had been noticed, but in the opposite direction, and not only in the lines of hydrogen, but also in those of the group δ and in the coronal line 1474 K, the displacement of the latter line being most unmistakably observed. Continuing his observations on July 21 and 22, M. Thollon found that the southern half of the sun showed manifest signs of violent agitation. The C line in this disturbed region was no longer a continuous dark line, but was broken up into a number of fragments, some bright, some very dark, which were often very largely displaced to right or left, and widened out, and diffused at the edges, particularly in the neighbourhood of the spots. At 7^h 33^m on the morning of July 22 a small prominence on the western limb attracted M. Thollon's attention by its brilliancy and a great displacement of the C line. An examination of the rest of the spectrum showed that a number of metallic lines were very strongly reversed, the D lines being especially brilliant and showing a double reversal at the base of the prominence. Clouds prevented more than momentary glimpses of the sun being obtained, and only the following lines were identified of those seen to be reversed, viz. λ 5189, 5197, 5274, 5323, 6673, Tacchini's red line, 1474 K, b_1, b_2 , the least refrangible component of b_3 , and the most refrangible of b_4 . M. Thollon states that he had never seen so brilliant a reversal of the metallic, and particularly of the D lines, as on this occasion. Larger displacements he had indeed seen, but never so many in so short a space of time.

THE Annual Meeting of the Astronomische Gesellschaft will be held this year in Vienna, in the apartments of the Academy of Sciences, from Friday, Sept. 14, to Monday, Sept. 17. Prof. Auwers is the President, Prof. Schönfeld the Secretary.

A NEW Minor Planet of the ninth magnitude was discovered on Aug. 13, by Prof. C. H. F. Peters, Director of the Clinton Observatory.

* Comptes Rendus, Vol. xcvii. No. 4.

Ephemeris of the Great Comet, b 1882.*

By H. KREUTZ.

There is a possibility that the great September comet of last year may be seen by means of our more powerful telescopes during the absence of the Moon from the morning heavens. Its brightness will be 0.05 of that which it showed on Feb. 8, when Dr. Schmidt, at Athens, last saw it with the naked eye, or about one third of its brightness on May 6, when it was last observed by Mr. Maxwell Hall at Kempshot, Jamaica. Mr. Hall found on that occasion a very close agreement between the observed place and that computed from Prof. Kreutz's elements.

For Berlin Midnight.

1883.	R.A.	Dec.	Log Δ.	Log r.
	h m s	° ' "		
Aug. 31	7 27 4.7	— 8 46 54	0.7769	0.7326
Sept. 1	7 27 26.1	8 51 50		
2	7 27 47.0	8 56 48		
3	7 28 7.5	9 1 48		
4	7 28 27.4	9 6 51	0.7768	0.7359
5	7 28 46.8	9 11 56		
6	7 29 5.7	9 17 3		
7	7 29 24.1	9 22 12		
8	7 29 41.9	9 27 23	0.7765	0.7392
9	7 29 59.2	9 32 56		
10	7 30 15.9	9 37 52		
11	7 30 32.1	9 43 10		
12	7 30 47.7	— 9 48 29	0.7760	0.7432

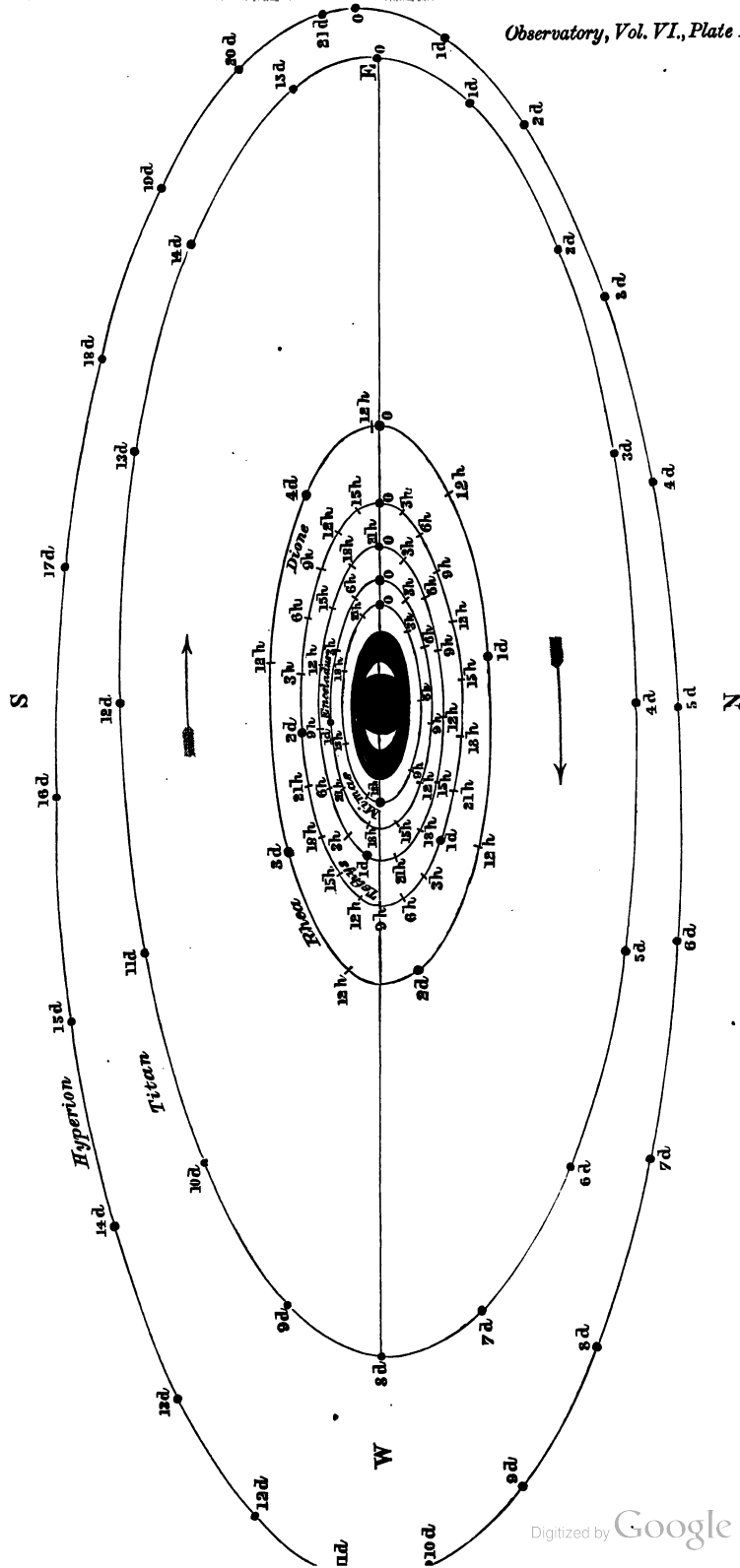
Differences of Right Ascension and Declination between Iapetus and the centre of Saturn. By A. MARTH*.

Greenwich Noon.	α—A. s	δ—D. "	Greenwich Noon.	α—A. s	δ—D. "
Sept. 1	—33.7	—164	Sept. 16	+ 1.7	—148
2	—32.5	—171	17	+ 4.7	—138
3	—31.1	—177	18	+ 7.8	—127
4	—29.5	—183	19	+ 10.8	—115
5	—27.7	—187	20	+ 13.7	—102
6	—25.7	—189	21	+ 16.6	— 89
7	—23.5	—191	22	+ 19.4	— 75
8	—21.1	—191	23	+ 22.1	— 60
9	—18.6	—190	24	+ 24.6	— 45
10	—16.0	—187	25	+ 27.0	— 30
11	—13.2	—184	26	+ 29.3	— 14
12	—10.4	—179	27	+ 31.4	+ 2
13	— 7.4	—173	28	+ 33.3	+ 17
14	— 4.4	—166	29	+ 35.0	+ 33
15	— 1.4	—158	30	+ 36.6	+ 49

* Astron. Nachr. No. 2527.

† Monthly Notices, Vol. xliii. No. 8.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



Observatory, Vol. VI., Plate I.

The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East elongation.

Sidereal Time at Mean Noon :—Sept. 1, $10^h 41^m.2$; Sept. 11, $11^h 20^m.7$; Sept. 21, $12^h 0^m.1$; Sept. 30, $12^h 35^m.6$.

Moon.		sets.		sets.		rises.	
		h	m	h	m	h	m
Sept. 1..	6	17		Sept. 11..	12	1	
2..	6	39		12..	13	7	
3..	7	3		13..	14	19	
4..	7	25		14..	15	36	
5..	7	49		15..	16	55	
6..	8	16		16..	rises.		
7..	8	49		17..	6	14	
8..	9	26		18..	6	44	
9..	10	9		19..	7	18	
10..	11	1		20..	7	57	
						30..	
Sept. 21..	8	44					
	22..	9	37				
	23..	10	36				
	24..	11	40				
	25..	12	47				
	26..	13	54				
	27..	15	1				
	28..	16	6				
	29..	17	12				

New Moon, Sept. 1, $2^h 14^m$; First Quarter, Sept. 9, $6^h 38^m$; Full Moon, Sept. 16, $9^h 41^m$; Last Quarter, Sept. 23, $0^h 51^m$; New Moon, Sept. 30, $17^h 54^m$.

Mercury is an evening star in Virgo, and is unfavourably situated for observation. He is at his greatest elongation, $26^\circ 42'$ E., Sept. 10, 17^h ; and is stationary, Sept. 23, 23^h .

Venus is in superior conjunction with the Sun Sept. 20, 11^h , and in conjunction with Uranus Sept. 17, 4^h . Diameter :—Sept. 1, $9''.6$; Sept. 30, $9''.6$. Illuminated portion of disk, 1.000 on Sept. 15.

Sept. 1, R.A. $10^h 23^m.3$, Dec. $11^\circ 30'$ N., tr. $23^h 43^m$, rises $16^h 42^m$
30, 12 36 .9, 2 36 S., 0 1 sets 5 52

Mars is in Gemini. Diameter :—Sept. 1, $6''.8$; Sept. 30, $7''.6$. Illuminated portion of the disk, 0.894 on Sept. 15.

Sept. 1, R.A. $6^h 23^m.0$, Dec. $23^\circ 38'$ N., tr. $19^h 41^m$, rises $11^h 23^m$
30, 7 37 .6, 22 23 N., 19 1 10 52

Jupiter enters Cancer early in the month. Diameter :—Sept. 1, $31''.0$; Sept. 30, $33''.4$.

Sept. 1, R.A. $7^h 50^m.4$, Dec. $21^\circ 12'$ N., tr. $21^h 6^m$, rises $13^h 6^m$
30, 8 11 .1, 20 17 N., 19 33 11 38

Saturn is in quadrature with the Sun, Sept. 2, 4^h , and is stationary, Sept. 22, 6^h .

Sept. 1, R.A. $4^h 33^m.5$, Dec. $20^\circ 3'$ N., tr. $17^h 50^m$, rises $9^h 56^m$
30, 4 35 .0, 20 2 N., 15 57 8 4

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	
Sept. 16	42'' .62	18'' .65	28'' .34	12'' .40	16'' .9
Oct. 6	44 .14	19 .28	29 .35	12 .82	17 .6

The south side of the rings is visible, the elevation of the Earth above their plane being $25^\circ 57'$ on Sept. 16, and $25^\circ 54'$ on Oct. 6, and of the Sun $25^\circ 24'$ and $25^\circ 31'$.

Uranus is in conjunction with the Sun, Sept. 16, 12^h.

Neptune is in Aries.

Sept. 1, R.A. 3^h 16^m·5, Dec. 16° 19' N., tr. 16^h 33^m, rises 9^h 3^m
30, 3 15·2, 16 13 N., 14 37 7 8

Phenomena.

	G. M. T.	
	h m	
Sept. 1	14 37	J. iii. Tr. I.
2	16 5	J. iv. Oc. R.
3	14 59	J. ii. Ec. D.
5	14 19	J. i. Ec. D.
	14 42	J. ii. Tr. E.
6	14 44	J. i. Tr. E.
12	14 33	J. ii. Tr. I.
	16 12	J. i. Ec. D.
13	14 23	J. i. Tr. I.
	16 43	J. i. Tr. E.
14	8 44	c ¹ Capricorni
		Oc. D. 185°.
	8 47	c ² Capricorni
		Oc. D. 104°.
	14 0	J. i. Oc. R.
16	17 7 25	Piscium
		Oc. D. 143°.
19	16 27 60	Piscium
		Oc. R. 236°.
19	13 22	J. iii. Oc. D.
	16 50	J. iii. Oc. R.

	G. M. T.	
	h m	
Sept. 20	11 47	B. A. C. 1119
		Oc. D. 45°.
	16 21	J. i. Tr. I.
21	14 37	J. ii. Oc. R.
	14 58	B. A. C. 1468
		Oc. D. 139°.
	15 53	B. A. C. 1468
		Oc. R. 236°.
	15 58	J. i. Oc. R.
22	13 10	J. i. Tr. E.
23	14 15 26	Geminorum
		Oc. D. 63°.
	15 16 26	Geminorum
		Oc. R. 294°.
24	13 19 68	Geminorum
		Oc. R. 283°.
26	12 58	J. iii. Ec. D.
	16 12	J. iii. Ec. R.
28	14 27	J. i. Ec. D.
29	12 48	J. i. Tr. I.
	15 8	J. i. Tr. E.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

Publications received:—J. A. Westwood Oliver, *Sunspottery, or What do we owe to the Sun?* (Simpkin, Marshall & Co., London)—Rev. S. J. Perry, *Stonyhurst College Observatory, Results of Meteorological and Magnetical Observations*, 1882 (Manresa Press, Roehampton)—August Tischner, *The Sun changes its position, therefore it cannot be regarded as being "in a condition of rest"* (Gustav Fock, Leipzig)—*Liverpool Astronomical Society, Abstracts of Proceedings*, Vol. i. Session 1882–83 (Meek, Thomas & Co. Liverpool)—Rev. T. E. Espin, *The Comet of 1882* (Trans. of Liverpool Astron. Soc. Vol. i.)—A. Gaillot, *Changements produits sur la durée de l'année julienne par les variations des quantités dont dépend cette durée* (Comptes Rendus, Vol. xcvii. No. 3).

ERRATUM IN No. 76.

Page 255, line 23, for 1883 read 1863.

The same misprint is made in M. Gaillot's paper in the 'Comptes Rendus,' Vol. xcvi. p. 153.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 78.

OCTOBER 1.

1883.

*Mountain Observatories *.*

MUCH attention has recently been directed to the question whether the conditions are more favourable to astronomical observations on the summit of a lofty mountain than at the level of the sea. The evidence so far collected is somewhat contradictory, although there can be no doubt that for certain investigations great elevations are almost essential. It therefore appears to be of interest to consider what advantages are to be expected, what work should be undertaken, and what instruments are best adapted to securing results of value. Astronomical science is at present limited not so much by the imperfections of our instruments as by meteorological conditions. The first cause of error is the change due to the effect of variations of temperature in the instruments themselves. This is especially noticeable in large reflecting telescopes, where a slight irregularity of temperature (such as would be produced by placing the hand upon the mirror) would entirely destroy the sharpness of the image. In any precise measures care must be taken that all portions of the instrument are at the same temperature, or serious deviations will ensue. The second and more important source of error is that due to the atmosphere. Owing to variations of temperature, the density, and consequently the index of refraction of the air, is constantly changing. This effect is magnified by a telescope, so that we always perceive the fluctuations in the images of a large telescope, such as are sometimes noticed by the unaided eye when looking through the column of heated air rising from a chimney. The effect of the absorption of the air is not very serious for objects near the zenith. At the level of the sea, about one quarter of the light of a star in the zenith is absorbed by the air. At the greatest elevations at which an observatory could be erected, only about one half of the air would be surmounted. The brightness of zenith stars will therefore only be increased a little more than a tenth of a stellar mag-

* Appalachia, iii.

nitude,—an amount which is scarcely perceptible by the most careful comparisons. In observing the Sun, the intense light of the sky in its immediate vicinity often seriously interferes with the results. The observations of our fellow-member, Prof. Langley, and of Prof. Young, have conclusively shown the advantages, in such researches, of very elevated stations. The light surrounding a bright star or planet is largely due to the aberration, internal reflections, and imperfect transparency of the object-glass. It is not certain, therefore, that any great benefit would be gained by an increased altitude in observing the fainter satellites, or companions to bright stars. The principal advantage we should anticipate would therefore consist in the increased steadiness of the images. Should this result be realized, the importance of a mountain observatory would amply repay the inconvenience of conducting it. Not only would the accuracy of all measurements be increased, but close double stars would be more easily separated, and the structure of the surfaces of the planets would be more distinctly shown. Stars too faint to be seen under ordinary conditions would become visible, owing to the concentration of their light in a single point.

We do not yet know whether more hours of good “seeing” can be obtained at a great height than at a properly selected point near the level of the sea. There can be no doubt that great advantages would accrue from a proper location; but it almost always happens that political or personal reasons determine the place where a large telescope is to be erected, independently of the best climatic conditions.

The difficulties of maintaining a large observatory at a great elevation are very serious. Among them may be named the expense of transportation of all the supplies needed, the unwillingness of observers to lead so isolated a life, its probable unhealthiness, and the difficulty of performing much work of any kind in a rarefied atmosphere.

The expenditure required for an observatory may be divided into the original cost of buildings and instruments, and the current expenses for making the observations, reducing them, and publishing the results. If architectural effect is not aimed at, the building expenses need not be large. The impression is prevalent that the principal expenditure should be made on the instruments. In establishing many observatories this has proved to be a fatal mistake. If much work is to be done, by far the largest appropriation should be made for current expenses, mainly for the salaries of a large corps of assistants. At the Harvard College Observatory, the current expenses for two years would cover the entire cost of the instruments. In other words, estimating the rate of interest at 5 per cent., ten times as much is expended on the observations, reductions, publications, and other current expenses as on the instruments. A similar remark applies to the observatories at Greenwich and Washington. In most forms of

routine work, such as is done at these observatories, the time required to prepare the observations for printing may be estimated at five to ten times that required to make them. The relative expenditures would not be so great, since much of the reduction consists in copying and in simple computations, which can be done by less expensive assistants. Apart from the high cost of living on the summit of a mountain, high salaries must be paid to assistants, to induce them to make the sacrifices required in such a life. Obviously, then, great economy may be attained by restricting the work on the mountain to that which can be done there only, and conducting the greater portion at some lower elevation, where the best facilities exist for completing it. Among other advantages will be the convenience for supervision and direction of the work, rapidity in publication, and ready communication with other observatories.

The principal question to be determined appears to be whether the increased steadiness of the air will give a real advantage. For this purpose two similar instruments, which need not be of very large size, should be erected, one on the mountain, the other at some convenient point below. Similar work should be done with each for one or more years. The results would show the relative advantages of the two stations.

To proceed now to details. A form of telescope described in the Proceedings of the American Academy* has especial advantages for mountain observa-

tions. The telescope A B (fig. 1) is mounted horizontally, pointing east or west, and has a plane mirror, C, inclined at an angle of 45° to its axis, placed in front of it. This

mirror can turn around an axis coinciding with the axis of the telescope, so that any object when crossing the meridian can be brought into the field of view.

The position of the mirror can be controlled and determined by long rods extending to the eye end. Objects can be observed only when within an hour or two of the meridian, but in any large piece of work there are always enough objects so situated. The usual method of erecting a telescope of the same

size in a dome is shown for comparison in fig. 2. Among the advan-

Fig. 1.

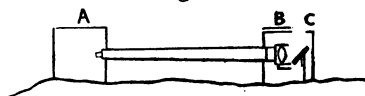
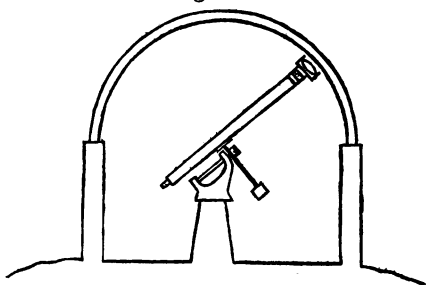


Fig. 2.



* Vol. xvi. p. 64.

tages of the horizontal mounting may be named the following:—A much smaller portion of the rock surface need be prepared for its erection. Only a small shed, which should be a part of the dwelling-house of the observer, and a pier at B C, covered to protect the lens and mirror from the weather, are needed. The intermediate space below the tube may be left in its natural condition. A dome requires a prepared circular floor of a diameter somewhat greater than the length of the telescope. To derive the full benefit from a dome, the view should be unobstructed. The observatory must be on the summit of the mountain, where it is greatly exposed to storms, and where fuel and water are difficult to obtain. The horizontal telescope, on the other hand, only requires a clear southern horizon. It may therefore often be mounted where it will be protected from the most violent storms. The wind is a serious source of danger to a large dome. The figures show the relative surfaces exposed to it. Snow and ice are liable to render it impracticable to use a dome; while with a horizontal telescope this difficulty is reduced to a minimum, since it is only necessary that the shutter, over C, may be opened. A most important advantage of the horizontal telescope is the convenience to the observer. The consequence is that the hours of observation may be prolonged, and far more work accomplished. In a rarefied air, where every movement is fatiguing, the labour of moving a dome would be very great. With a horizontal telescope, as the eyepiece does not move, the observer sits in comfort, always looking horizontally. One of the greatest advantages is that the observer may work in a warmed room. The object-glass is so far distant, horizontally, that the heat at the eyepiece will not affect the definition. Work may thus be carried on with perfect comfort on the coldest nights. The application of this to observations during the prolonged nights in the Arctic regions is obvious. Portability is often a serious consideration in work at great altitudes. No portion of the horizontal telescope need be so heavy but that it could be carried by a horse, or even by a man. The tube need only support its own weight, and may be made of tin. A heavy steel tube is required to give sufficient stiffness with an equatorial mounting. The horizontal telescope has especial advantages in steadiness, as mirror, object-lens, and eyepiece are all close to the supporting pier. This is an important consideration on windy nights.

Various researches suitable to a horizontal telescope have been suggested in the article named above. In general, the greatest proportionate saving will be effected when the least skill and time are required in the observations as compared with their reduction. Photography offers an especially promising field in this respect. The improvements in dry plates render it not improbable that photographs will replace star-maps for the brighter, and perhaps for the fainter, stars. A skilful photographer, even if he had but little knowledge of astronomy, might obtain in a short time a collection of photographs which, by a proper discussion in a more

convenient station, would yield a vast collection of valuable results. It cannot be long before a daily photograph of the solar protuberances, with occasional photographs of the solar corona, will form a part of the routine work to be expected from astronomers. The advantages of a great elevation in such work are undoubted.

The working hours of the assistants should be devoted almost entirely to observations. But little of the reductions should be attempted, as this can be much better done at the central office. A sufficient description of the work should of course be appended, to render the results intelligible to another person; but almost all the clerical work can be better done below.

Of course many meteorological observations could be taken with advantage in a mountain observatory, and by self-registering instruments the time of the observer could be saved. The maximum force of the wind in storms, and the minimum temperature in winter, should be especially noted. An important investigation would be the measurement of the variations in the atmospheric refraction by the micrometer level (*'Appalachia,'* i. pp. 63, 138). The apparent altitude of a number of the most distant points should be observed from time to time, and the atmospheric refraction deduced. Observations of the thermometer and barometer should be obtained at the same time. The value of the work would be greatly increased by simultaneous observations at one or more of the points observed. Such observations might at least be obtained at some point in the country below. Barometric observations at such a point, where the difference in height was as great as possible and the horizontal distance small, would have great value in determining the relation of the barometer to the height, as affected by other meteorological conditions.

In selecting a proper location, many preliminary observations would be necessary. We cannot depend altogether on the meteorological observations already made, as they commonly give the rainfall rather than the amount of cloudy weather. If a point could be found where the sky was almost always clear, at least in certain seasons, a great additional advantage would be gained for many purposes. Much time is now spent in preparing to observe certain occasional phenomena, and no result obtained on account of clouds. With a certainty of clear weather, one observatory could often make all the observations needed of such phenomena.

The portability of the horizontal telescope is a strong argument in favour of its use in the preliminary observations. The first trial might be made with a telescope of the usual form, small enough to be carried by a horse. As high a magnifying-power as it would bear should be employed, and images of bright stars, of close doubles, and of faint companions to bright stars should be studied as they approach the western horizon. By noting the time at which the images become unsatisfactory, we can compute the altitudes at which they are equally good on different nights. We have thus a quantitative test of the steadiness of the atmo-

sphere in different places. The amount of air looked through is proportional to the secant of the zenith-distance. Suppose that at a given location we could see a star to within 30° of the horizon, as well as we could see one in the zenith at the sea-level. Since the zenith-distances in the two cases are 60° and 0° , and the secants 2 and 1, we should infer that the relative steadiness of the air at these places was as 2 to 1. Repeating the observation with a number of stars, we should soon establish a habit of observing which would give a much better determination of the condition of the air than any ordinary estimate. The transparency of the air may also be well determined by the eye alone. It is only necessary to compare a bright star, while rising or setting, with others near the zenith apparently equal to it in brightness, and note the time of such comparison. A watch and star-map are all that are needed for these observations. The difference in the absorptions will then equal the true difference in brightness of the stars. The reduction may be made subsequently by computing the altitude of each star, and deducing the law of absorption by combining all the observations made at nearly the same altitude. These observations are so easily made that they are recommended to any traveller in a region where the atmosphere appears to be particularly clear.

So much time will inevitably be occupied by the preliminary observations, that some years must elapse before we can expect to find a mountain observatory in full operation. An important step would be taken by the Club if it could lessen this interval by aiding in the preliminary work. No mountain in this vicinity is sufficiently high to be used for such an observatory as is here contemplated. This objection would not, however, apply to the preliminary observations. Mount Washington would not be suitable on account of the prevalence of clouds and the severity of the winds and storms. The same criticism may be made to the selection of Mount Lafayette, Mount Adams, or Mount Jefferson. Their pointed summits, and the difficulty and expense of occupying these stations, also render them undesirable. Mount Moosilauk, on the other hand, although less elevated, has a large flat summit, well adapted to work of this kind. It is, moreover, within convenient reach of the railroad, and has a carriage-road to the top, over which the instruments could easily be transported. The house on the summit would afford abundant accommodation for observers, and has already been occupied during the winter by a member of this Club. Probably no point could be found, having an equal elevation, where the preliminary observations could be conducted at less expense. Several boarding-houses at the base of the mountain would furnish additional accommodation, if needed, and greater comfort in case of accident or illness. Should any unforeseen difficulty present itself, nearly equal advantages would be possessed by Mount Mansfield, the highest of the Green Mountains. A series of observations at one of these points would clearly show the difficulties to be anticipated at a more elevated station. For

the latter it is doubtful if we could find a better point than Mount Whitney, the station selected by Prof. Langley. The geographical location and meteorological conditions of this mountain are particularly favourable; it is one of the highest summits in the United States, and is so situated as to be unusually free from haze, clouds, and storms. Although at present somewhat difficult of access, it is probable that a railroad will soon be constructed to within a short distance of its base.

In conclusion, it is still uncertain whether any important advantage would be gained in most astronomical work by a great elevation, as compared with the best possible location at a moderate height. The question should, however, be decided, and it is believed that by the plan here proposed a sufficient test could be obtained at small expense. In any case valuable results would be attained, even if the observations in the observatories on the mountain summit were not much better than those at a less altitude.

EDWARD C. PICKERING.

Sir W. Siemens on the Conservation of Solar Energy.*

IN the present volume Sir W. Siemens has published his original paper on the conservation of solar energy read before the Royal Society, 1882 March 2, with the principal criticisms his theory called forth, and the replies he made to them. He has somewhat supplemented his original paper by passages from the article he contributed to the 'Nineteenth Century,' and has also added an extract from his Presidential Address to the Meeting of the British Association at Southampton, 1882 August, in which he adduced some further considerations in support of his theory. The book therefore presents a very complete view of the whole question, and scientific men will be glad to have a discussion which has occupied so many able minds laid before them in its entirety.

A full abstract of the original paper having been given in the 'Observatory' for 1882 May, and the theory being sufficiently widely known from Sir W. Siemens's own article in the 'Nineteenth Century,' and from the notice it has so widely attracted, it will suffice here to briefly allude to its principal features. These are thus stated by the author himself, p. 26:—

"1. That aqueous vapour and carbon compounds are present in stellar and interplanetary space.

"2. That these gaseous compounds while in a state of extreme attenuation are capable of being dissociated by radiant solar energy.

"3. That the effect of solar rotation is to draw in dissociated vapours upon the polar surfaces and to eject them, after combustion, back into space equatorially."

* On the Conservation of Solar Energy. A Collection of Papers and Discussions. By C. W. Siemens, F.R.S., D.C.L., LL.D., Ph.D., Mem. Inst. C.E. With Illustrations. London: Macmillan & Co., 1883. 20+111 pp. 8vo.

In this mode Sir W. Siemens supposed that the whole or nearly the whole of the solar radiations not received by the planets might be utilized and eventually returned to the Sun ; and "our solar system would no longer impress us with the idea of prodigious waste through dissipation of energy in space, but rather with that of well-ordered self-sustaining action, capable of perpetuating solar radiation to the remotest future."

This ingenious theory is defended by its author with such earnestness and ability, and with such fertility of resource, that an astronomer can scarcely fail to feel regret that he finds himself unable to accept it. It originated in the laboratory experiments of one of the ablest of living electricians, experiments of the greatest interest and importance, but it supposes a condition of things in and around the Sun of which the telescope and spectroscope afford us no indication. We know nothing of any interplanetary atmosphere, but are, on the contrary, assured that no retardation of planetary motion, such as even a very attenuated atmosphere would produce, has yet been detected. "The fan-like action resulting from the rotation of the Sun," on which the entire theory depends, is not very easy to understand ; but neither telescopic examination of sun-spots, nor spectroscopic examination of the prominences and chromosphere, has shown any thing like the steady current flowing from the poles to the equator which is involved in it. Some of the sketches of the corona made during the 1878 eclipse do indeed resemble the diagram Sir W. Siemens drew to illustrate his theory ; but there was, on the one hand, no reason apparent why the currents in these attenuated compound vapours should be visible to us, and, on the other, the corona in years nearer the period of maximum solar activity has always presented an entirely different aspect to that which it did in 1878.

It is, however, when we look at the main purpose of the theory that its full weakness becomes apparent, that purpose being, in Sir W. Siemens's own words, to show how "the radiant energy which is now supposed to be dissipated into space and irrecoverably lost to our Solar System, could be arrested and brought back in another form to the Sun itself, there to continue the work of solar radiation." It therefore involves that the *whole* of the solar radiations must be absorbed within some distance from the Sun so comparatively short that the "fan-action" can draw the interplanetary atmosphere inwards towards the sun. Beyond this distance therefore the Sun would be invisible ; and Fitzgerald's question, "why we can see the stars, although most of the solar radiations are absorbed within some reasonable distance of the Sun," is a most pertinent one, and, indeed, Dr. Siemens's answer is in effect a surrender of the whole question. "Some rays," he says, "are more easily absorbed than others ; thus it appears to be the yellow ray which is most efficacious in the decomposition of carbonic acid and aqueous vapour in the vegetable cell. May not the same conditions prevail in space and allow probably the rays of highest refrangibility to pass on to

the greatest distance without being absorbed ? ” In answer to this it is sufficient to say that we have no evidence in the colour of the stars of the existence in space of any such selective absorption ; and it is very clear that if such were the case, the conservation of solar energy would still be an unsolved problem, for the unabsorbed radiations would represent an enormous amount of energy which would be “ irrecoverably lost to our solar system.” Similarly, when Dr. Siemens refers to Captain Abney’s observations as to the existence of hydrocarbons in interplanetary space, as confirmatory of his theory, he overlooks the fact that the evidence on which Captain Abney bases his inference is not a strong and general absorption, but an absorption which, as compared with the whole range of solar radiation, is weak, special, and restricted. Further, we receive from certain stars, such as Arcturus and Sirius, so much light and heat as to prove, without allowing for any absorption, a greater total original radiation from those stars than from our Sun ; so that even if we could accept the theory as true with respect to the latter, we should simply have to transfer this problem of the apparent dissipation of energy from the solar to the sidereal systems. But we have no evidence that any considerable absorption takes place even within the solar system. The four outer planets shine so brightly that it is plain that no serious proportion of the Sun’s light has been absorbed ere reaching them. The albedo of Jupiter is particularly high ; and with this planet we have the additional assurance, derived from the invisibility of its satellites during eclipse, that no sensible part of the planet’s light is intrinsic to itself. There is but one region of the solar system where we reasonably conceive of any great absorption taking place, and that is the region interior to the orbit of the Earth. But if any such action takes place there, it is wholly irrelevant to the problem before us, since by how much we suppose the solar radiations to suffer by absorption before reaching us, by so much must we increase our notions of the original emission.

We may therefore confidently assume that the riddle as to what becomes of all but an infinitesimal proportion of the solar radiation remains as completely unsolved as ever.

The details of the theory may possibly have some correspondence with fact ; there may be hydrocarbons in space, and these may suffer dissociation in space, and there may be a continual inflow of dissociated gases at the solar poles, and as constant an outflow of compounds from the equator ; but the admission of all these postulates will not seriously affect the main question ; it is not thus that we can explain the conservation of the solar energy. Nevertheless that so important a problem should have been attacked from so novel a standpoint will not have been without its advantages ; and Sir W. Siemens’s theory will at least, in the words of the late lamented President of the Royal Society, “ ever be remembered as having set many active minds at work, and will always have a place in the history of solar physics.”

CORRESPONDENCE.

*To the Editor of 'The Observatory.'**Determination of Local Longitudes in the Indian Ocean and Australia.*

SIR,—

I have lately received from Mr. Todd, of the General Post Office, Adelaide, the following important information on the determination of local longitudes in the Indian Ocean and Australia.

The White House, Greenwich Park,
1883, Sept. 12.

G. B. AIRY.

“Communication of CHARLES TODD, Esq., dated General Post Office, Adelaide, June 21, 1883.

“You will be glad to hear that the longitude business has been satisfactorily completed as far as New Zealand. As I could not myself go to Port Darwin, Mr. Ellery and I selected a Mr. Barrachi, a talented young Italian observer; and we arranged with him to exchange signals with Captain Darwin at Singapore and with Captain Helb at Banjoemanjie on the one side, and with me at Adelaide and Mr. Ellery at Melbourne on the other side. It was also intended that he should exchange signals with Mr. Russell at Sydney; but this, for want of time, could not be accomplished.

“Barrachi and Captain Darwin observed together and with Mr. E. J. White at Melbourne, for personal equation, and both observed at Sydney with Russell. Barrachi and I also observed together, when I was recently in Melbourne on my way from a postal conference in Sydney.

“The following are the results:—

True Longitude of Singapore by Oudemans in 1870.

	h	m	s
Flagstaff, Government Hill.....	6	55	22.714
Reduction to Captain Darwin's station			+1.510
	6	55	24.224

Differences of Longitude determined by Captain Darwin at Singapore, Captain Helb at Banjoemanjie, Barrachi at Port Darwin, C. Todd at Adelaide, R. L. J. Ellery and Mr. E. J. White at Melbourne, and H. C. Russell at Sydney:—

	h	m	s	s
Singapore—Port Darwin	1	47	57.512	±0.04
Port Darwin—Adelaide	0	30	57.879	±0.030
Adelaide—Melbourne	0	25	33.737	±0.015
Port Darwin—Melbourne	0	56	31.640	±0.030

	h	m	s
Longitude of Singapore	6	55	24.224
Singapore—Port Darwin	1	47	57.512
Longitude of Port Darwin	8	43	21.736
Port Darwin—Adelaide	0	30	57.879
Longitude of Adelaide	9	14	19.615
Adelaide—Melbourne	0	25	33.737
Longitude of Melbourne	9	39	53.352
Melbourne—Sydney	0	24	55.098
Longitude of Sydney	10	4	48.450

Port Darwin to Melbourne.

	h	m	s
By direct signals	0	56	31.600
Through { Adelaide—Melbourne .. 0 25 33.737 }	0	56	31.616
Adelaide { Adelaide—Port Darwin. 0 30 57.879 }			

"The signals consisted of automatically-transmitted clock-beats recorded on the chronograph at each station. As a rule, each clock transmitted two batches of signals, each batch extending over $2\frac{1}{2}$ minutes, *i. e.* commencing at 45° and ending at 15° .

"These results are subject to small corrections, on receipt from Captain Darwin of small errors not yet taken into account.

"I have only just received Captain Helb's observations, and cannot yet give the results affecting Banjoemanjie. Captain Helb exchanged signals with both Singapore and Port Darwin.

"Mr. Barrachi has been intrusted with working out the final results; and Ellery and Russell and myself will furnish a joint report.

"Colonel Tupman exchanged signals with Russell before he left New Zealand. I am sorry I did not see him, but I was away on a cable-laying expedition when he arrived in mail-steamer.

"My new building for the Transit Circle is nearly finished, so I hope to mount the instrument in a few weeks. (Signed) C. Tonn."

Saturn's Ring.

SIR,—

With reference to Prof. Bakhuyzen's letter in the July number of 'The Observatory' (vol. vii. p. 217), perhaps you will allow me to point out that his reference to Campani's drawing of Saturn and its ring in 1664 is not new, but that Dr. Hind has a paper on the subject in the 15th volume of the 'Monthly Notices.'

A note is subjoined by the editor, in which reference is made to an original representation of the planet and its appendage by Campani, in the MS. Letter Book of the Royal Society (vol. i. p. 324). By the kindness of Mr. Herbert Rix, I have been enabled to refer to this drawing, which was made on October 5th, 1665. It is on a slip of paper carefully inserted in the volume, and accompanies a letter from Auzout to Oldenburg, in which the writer states that it had been forwarded by Campani himself in a letter to Cassini. Auzout's letter is dated May 18th, 1666; and by the permission of the Secretaries of the Royal Society, I offer you the following extract from it, which is of great interest, and evidently alludes to the drawing of Saturn by the Balls, respecting which there has been so much discussion since I wrote to you on the subject about this time last year. Auzout speaks first of Cassini's recent discovery of a permanent spot on Jupiter, refers to Hooke's observations of that planet, and then says:—

“ Nous verrons cet été, si de si petites lunettes suffiront pour confirmer la belle decouverte de Cassini. J'avoue, qu'il faut que sa lunete soit excellente, et meilleure sans difficulté pour sa longueur, que celle avec laquelle on a fait l'observation de Saturne, que vous avez mise dans votre dernier Journal.” [This clearly alludes to the drawing of Ball's on October 13th, 1665, in No. 9 of the 'Philosophical Transactions,' for there is no drawing of Saturn in any preceding number up to the time of Auzout's letter; this drawing was therefore in the copy which Auzout had seen.] “ J'ay été fort surpris de voir cette figure*. Tout ce que je vous puis dire, c'est que n'ayant pas observé le mois d'Octobre à cause de mon absence, toutes les fois j'ay observé Saturne dans le mois de Septembre, qu'il devoit estre encore un peu plus large; je ne l'ay observé que sous la forme d'une parfaite ovale, un peu moins large que l'année précédente, mais sans aucun creux, et pour vous assurer d'avantage de la chose, je vous envoie la figure que Campani a fait imprimer dans une lettre qu'il a escrit à Cassini sur ses nouvelles decouvertes, et qu'il a prise le 5 Octobre, luy et plusieurs autres, entre lesquels il y avoit un excellent dessinateur; car je ne croy pas, que vos Messieurs voulussent, que depuis ce temps là, la figure de Saturne eust changé. Vous y verrez ce que vos Messieurs n'ont pas remarqué, et qui s'y devoit pourtant voir, à scavoir l'ombre du corps de Saturne sur son Anneau, et cela me fait craindre, que leur lunete ne fust pas excellente, car et Fontana et d'autres, qui n'ont pas eu d'assez bonnes lunettes l'ont souvent représenté comme cela, comme l'on peut voir et dans Fontana et dans les Figures que M. Hugens a recueillies dans son système. Vous pouvez scavoir de luy, de quelle figure il l'aura observé, et peut estre qu'il aura observé vers

* Before this sentence, Auzout appears to have erased some words (so carefully that “Saturne” only can be made out beneath the erasure); after it there is a clause, scarcely intelligible, containing the words “posée comme une observation,” and “y prendre garde,” and showing that he was wishing to express himself cautiously.

la fin d'Octobre; mais je vous puis assurer ici de M. Bouillaud qu'il l'a observé en parfaite ovale."

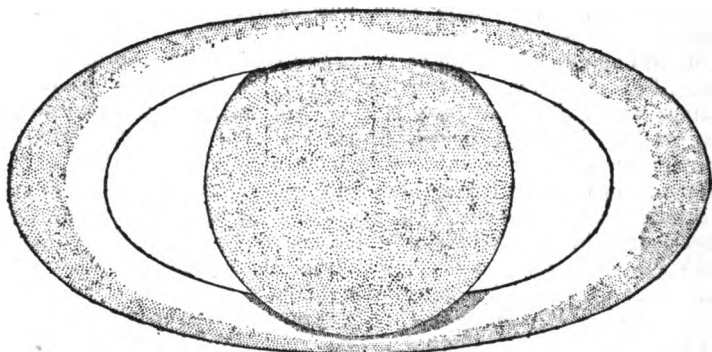
From Campani's drawing of Saturn on the 9th (nones) of July, 1664, Prof. Bakhuyzen has drawn the same deduction as the late Father Secchi did ('Monthly Notices,' vol. xiii. p. 248, for June 1853), that Campani perceived traces of the inner dark ring, usually supposed to have been first seen by Galle in 1838, and known, since Bond's perception of its true nature in 1850, under the technical name of the dusky ring. In the light of our present knowledge, this seems very probable; having no suspicion of any such thing, Auzout, as will be noticed above, thought that the phenomenon which Campani and others had seen, but which the Balls had failed to see, was merely the shadow of the body of Saturn on the ring.

On other occasions, subsequent to the observations of Campani, and before that of Galle in 1838, it has been thought that indications of the dusky ring have been noticed. Dr. Hind thinks (see the paper referred to above, 'Monthly Notices,' vol. xv. p. 31) that such were in fact seen by Picard in 1673. About fifty years after that time, some observations were made by Hadley (the famous inventor of the quadrant called by his name), which it will be interesting to quote here from a paper in the 'Philosophical Transactions' for 1723, containing the substance of certain Minutes of the Royal Society in which they are recorded.

Observing Saturn in the spring of 1720, at a time when the planet was about 15 days past the opposition, Hadley then "saw the shade of the Planet cast upon the Ring, and plainly discerned the Ring to be distinguished into two parts, by a dark Line concentric to the Circumference of the Ring. The outer or upper Part of the Ring seemed to be narrower than the lower or inner Part next the Body, and the dark Line which separated them was stronger next the body, and fainter on the outer Part towards the upper Edge of the Ring. Within the Ring he discerned two Belts, one of which crossed *Saturn* close to its inner Edge, and seemed like the Shade of the Ring upon the Body of *Saturn*; but when he considered the Situation of the Sun, in respect to the Ring and *Saturn*, he found that Belt could not arise from such a Cause."

In 1722 and 1723, Hadley stated that he was able to trace the dark line in the ring of *Saturn*, not only on the ansæ, but very nearly, if not quite, round the circumference; particularly in May 1722, "he could discern it without the Northern Limb of *Saturn* in that part of the Ring that appeared beyond the Globe of the Planet." And he remarked that the so-called Belt or dusky Line which attracted his attention in 1720 was still continued in 1722 and 1723 close to the inner edge of the ring across the disk, though the breadth of the ellipse had considerably increased since the former observation.

It is difficult to resist the conclusion that Hadley did in fact see



Campani's drawing of Saturn, 1664, July 9.

indications of what is now known as the dusky ring, and that he perceived (what appears to have escaped Campani and Auzout) that the phenomenon in question could not be produced by the shadow of the ball of Saturn falling on the ring. But to Campani must apparently be conceded the credit of first noticing the difference of brightness of the exterior and interior portions of the ring, or (as we should now call them) of the exterior and interior bright rings.

It is to be remarked that at the time of Campani's observation in 1664, and Hadley's in 1723, the respective positions of the Sun, Saturn and its ring would be very nearly the same.

Mr. Prince, of Crowborough Beacon, has a copy of Lubienietz's '*Theatrum Cometicum*;' and whilst I was recently staying a few days at his observatory, he took an excellent photograph of Campani's drawing of Saturn made on the 9th of July, 1664, which (as Prof. Bakhuyzen remarks in his postscript) is engraved in the '*Theatrum*.' This photograph Mr. Prince kindly permits me to send you with this letter, for insertion (if you think it desirable) in the next number of '*The Observatory*.'

Yours faithfully,

W. T. LYNN.

Blackheath, 1883, September 6.

Periodical Comets.

SIR,—

Mr. Lynn's notes ('*Observatory*,' Sept., pp. 269, 270) on Tempel's comets of short period are very interesting. In the case of the third comet discovered by Tempel on November 27, 1869, and at its last return by Lewis Swift on Oct. 10, 1880, there seems some difference of opinion as to whether the comet belongs to Tempel or Swift. Tempel can undoubtedly claim priority of discovery, though at the return of 1869 the periodicity of the comet escaped detection. Dr. Bruhns, however, published the elements of this comet (III. 1869) in the '*Astronomische Nachrichten*,' No. 1788, and called attention to the fact that his

observations indicated a variation from a parabolic orbit. In 1880 the periodicity of the comet was conclusively proved, and it seems therefore that Swift has the greater claim, seeing that he was its first discoverer at that return which led to the definite determination of its period. Moreover Tempel has already discovered two comets of short period and one of longer period, viz. (I. 1866) the derivative comet of the November Leonids returning every 33 years, so that we may well accord to Swift, in appreciation of his labours in this field, the chief credit of discovering the periodical comet of 1880. The naming of comets after their discoverers is adopted merely as a ready means of reference and distinguishing one from another, and not altogether as an honour to those individuals who either from intention or accident are sufficiently fortunate to be the first to alight upon these bodies.

As to Winnecke's periodical comet, it is well known that Pons discovered it on June 12, 1819, and that Encke ascertained its period, but that it escaped observation during the six returns to perihelion which must have occurred before that of 1858, when it was rediscovered by Winnecke, who pointed out its identity with Pons's comet of 1819, and closely corroborated the period formerly given by Encke*. It seems to me therefore that this comet should bear the name of Pons. Pons first discovered the comet, Encke computed the elliptical orbit, and if during many ensuing years it eluded rediscovery owing either to unfavourable position or insufficient observation, the fact of priority nevertheless holds good. Moreover on behalf of Pons it may be added that apart from being the most successful comet-hunter that ever lived, he was the first to discover Encke's comet in 1818, at which return its short period was determined. This notable comet had, it is true, been observed at three returns to perihelion prior to 1818, viz. in 1786, 1795, and 1805, but the true character of its orbit was not revealed until Encke undertook the discussion of the observations made in 1818. I suppose that in reference to the comet of 1819=1858 (Pons-Winnecke) it may be said that the observations of the former epoch, though enabling the true form of orbit to be ascertained, were yet insufficient to allow the comet to be re-detected at its subsequent returns. In fact this comet appears to have been practically lost until Winnecke in 1858, in the ordinary work of comet-seeking, alighted upon it again, and was enabled to compute the orbit with such precision that there is no probability the comet will evade observation at future returns, but must remain a durable and well-ascertained member of our system. But the priority of Pons and Encke in 1819 with regard to this comet is still significant, notwithstanding the more recent and perhaps more valuable labours of Winnecke in the same direction.

Yours faithfully,

W. F. DENNING.

Bristol, 1883, September 8.

* Encke's period was 5'618 years. Winnecke's subsequent determination gave 5'549 years.

The Tails of the Great Comets, I. & II., 1882.

SIR,—

Although during the appearance of the two great comets of 1882 my attention was chiefly directed to the determination of the positions of the nuclei, I availed myself of several opportunities for observing the tails. In the case of comet I. (Wells), I estimated as accurately as possible the positions of the extremity of the tail by alignments with certain stars, and then read off from a chart the places in right ascension and declination. The following table exhibits the results of these observations, the times of observation being given to the nearest hour :—

Date of Obs. 1882.	Position of Extre- mity of Tail.		Concluded Length of Tail.	Remarks.
	R. A.	Dec. N.		
d h June 19 6	h m 8 0	° ' / 4 45	° 14	Tail broad and beautifully curved, the convex margin facing north.
„ 20 6	8 15	7 30	12½	Tail still broad and curved, but neither of these characteristics so marked as on 19th.
„ 21 6	8 28	6 45	12½	Tail slightly curved, β Cancri being on its south margin, and the axis directed towards δ Hydræ.
„ 22 6	8 27	9 30	8½	Tail was nearly straight and pointed directly to ϵ Hydræ.
„ 23 7	8 47	7 30	10½	Tail not so curved, but very broad at its extremity. ϵ Hydræ was on its southern margin.
„ 24 6	8 56	8 0	10	Tail as on June 23.
„ 26 7	9 11	8 30	7½	ϵ Cancri on south margin.
„ 27 7	9 17	9 0	6½	Tail faint, owing to increased moonlight.
„ 28 6	9 34	9 0	8½	Faint, owing to same cause and slight haziness of sky.
July 3 7	10 0	9 15	4	} Faintly visible. Hardly distinguishable and appeared to involve A Leonis.
„ 4 7	10 3	9 30	3	
„ 5 7	..	..	..	

Unfortunately the Moon was increasing in brilliancy during the period of observation, and so prevented the tail being seen to advantage.

I measured, by means of a sextant, the apparent lengths of the tail of the great September comet, the results being highly satisfactory, owing to the abrupt and well-defined termination of the appendage. In some instances I obtained the length of the tail by reflecting to the nucleus a star near its extremity. The tail was remarkable for its great breadth and brilliancy, and its southern border, which was better defined than the northern, was prolonged several degrees beyond the main portion. This prolongation, which was certainly not half as broad as the main portion, was ill-defined, and is not included in my sextant measurements. On the morning of September 26 (*civil time*), the two margins of the

tail were so distinct and well defined, that I could very readily determine its breadth by double measures with the sextant; the breadth of the middle part of the main tail was found in this way to be $1^{\circ} 14'$. On this occasion the faint prolongation of the southern side of the tail extended as far as seven or eight degrees beyond the main tail. I also observed a dark, very narrow, and well-defined line extending from the comet's head to the point from which the prolongation started. It was parallel to, but on the south side of, the axis of the broad tail. This dark line was still visible on the morning of October 3. The following are the results of my sextant measures, the times of observation being given to the nearest hour and the lengths of the tail to tenths of a degree:—

	d	h	°		d	h	°
Sept.	23	17....	7.6	Nov.	9	14....	17.5
	25	17....	7.4		14	13....	20.0
	30	16....	12.1		20	15....	16.7
Oct.	2	16....	12.5		22	15....	17.5
	5	16....	14.1	Dec.	1	12....	13.5
	16	16....	16.2		8	10....	11.4
	19	16....	16.5		12	9....	7.2
	22	15....	15.9		14	11....	8.6
	30	16....	20.2		31	9....	6.7
Nov.	6	14....	17.9	Jan.	9	14....	4.5

The tail was estimated to be two or three degrees long on January 26; and the comet itself was last seen with the unassisted eye, and then as the faintest possible streak of light about two degrees long, on the evening of February 13.

Windsor, N. S. Wales,
1883, August 8.

I am, Sir, yours faithfully,

JOHN TEBBUTT

NOTES.

THE TOTAL SOLAR ECLIPSE OF 1883, MAY 6*.—On Sept. 3 M. Janssen reported to the French Academy on the results obtained by the Eclipse Expedition, which embraced several points of interest not given in the reports of the English and American parties. M. Janssen's own spectroscopic observations may be specially mentioned, since he obtained the clearest evidence that "the basis of the coronal spectrum was formed by the complete Fraunhofer spectrum," and that therefore "there exists in the corona, and, above all, in certain localities of it, an enormous amount of reflected light; and since we know that the coronal atmosphere is very rare, it follows that these regions must abound in cosmic matter in the state of solid corpuscles, in order to explain this

* Comptes Rendus, Vol. xevii. No 10.

abundance of reflected sunlight." This solar spectrum was most evident in the lower and more brilliant portions of the corona.

The French photographs of the corona appear to have been especially successful. They will of course require a most careful and detailed examination; but M. Janssen was able to state positively that they showed a much greater extension of the corona than was visible in the telescope or with the unassisted eye. Many of the large photographs are exceedingly well defined, and show important details in the structure of the corona. "The forms of the corona were absolutely unchanged during the duration of the totality."

An observation of M. Tacchini promises to be of the greatest importance; for he believed that he recognized in the spectrum of part of the corona two of the bands, that in the green and that in the yellow, so well known as characterizing the spectra of comets. Notwithstanding M. Tacchini's experience in spectroscopic work, it will of course be premature to consider the point established until confirmed by further observation, though at the same time we cannot say that there is any thing intrinsically improbable in it. It will, however, certainly become the duty of observers of future eclipses to sedulously follow up the question thus raised. Another important result has been the photometric measure of the luminous intensity of the corona, the first time a precise determination of it has ever been secured. The illumination given by the corona was found to be greater than that given by the full Moon. M. Palisa was able to pronounce in a very decided manner that there was no object near the Sun on the east, brighter than the fifth magnitude, which was not a well-known star. M. Trouvelot saw on the west an object which he had no opportunity of identifying; but Prof. Holden's examination of this region renders it all but certain that this was a known star. Astronomers generally will probably concur in M. Janssen's verdict, "that there is now very little probability that there are one or more planetary bodies of any importance between Mercury and the Sun." This conclusion, he adds, seems borne out by the photographs so far as they have yet been examined. It will be seen therefore that, notwithstanding the partial disappointment of the English observers through the nearly total absence of prominences, the eclipse of 1883 May 6 has already been very fruitful of noteworthy results, and it is possible that the further discussion of the photographs secured may furnish us with yet additional information.

M. Trouvelot has since carefully examined the region over which he swept during the eclipse, and picked up the two white stars he had noticed without any difficulty. These were α Arietis and ϵ Arietis. But the star "brillante, d'un rouge prononcé," seen during totality could not be found; and he is sure that no star of that colour and brightness now exists in or sufficiently near the position he had assigned to that object. But though this would seem to point to the conclusion that the object seen was really a

planet, M. Trouvelot, seeing that the essential elements of a determination of a position or of an appearance of a disk or sensible phase were lacking to his observation, considers it right to reserve his conclusion as to the probable nature of the object in question.

ASTRONOMY IN THE UNITED STATES.—The American Association for the Advancement of Science met at Minneapolis, August 15, under the Presidency of Prof. C. A. Young. In the section of Mathematics and Astronomy, the Vice-President of the section gave a very important address "On the German Survey of the Northern Heavens." Amongst the papers read before the section were the following:—The Total Solar Eclipse of 1883, May 6, by Prof. E. S. Holden. French Observations of the same eclipse, by Dr. J. Janssen. A new Method of Investigating the Flexure Corrections of a Meridian Circle; and, An improved Method of producing a Dark-field Illumination of Lines ruled upon Glass; both papers by Prof. W. A. Rogers. The light-variations of Sawyer's variable, by S. C. Chandler. The light-variations of T Monocerotis by E. F. Sawyer. Method of observing eclipses of Jupiter's Satellites by D. P. Todd. Other papers are referred to in the following notes.

The Rotation of Domes.—Dr. G. W. Hough of the Dearborn Observatory, in a paper on the Rotation of Domes, stated that he had employed a gas-engine for that purpose with most satisfactory results. Prof. C. A. Young, in discussing the paper, said that he had also been obliged to make use of a similar motive power at Princeton.

The Almacantar.—Mr. S. C. Chandler read a paper on Results of tests with the Almacantar in time and latitude, claiming for this instrument that a higher degree of accuracy is attainable by this instrument than by a transit or zenith telescope of the same size. The instrument consists of a trough containing mercury, and which revolves horizontally on an upright central pillar. In the trough is a float which carries a telescope. This turns on an horizontal axis and can be clamped at any altitude. The almacantar therefore is in its general nature an equal altitude instrument. Mr. Chandler considers that the principle of flotation adopted is as delicate an indication of the direction of gravity as is obtained by a spirit-level and that the almacantar promises to be a new instrumental resource in the higher practical astronomy, and that it will evade some of the minute sources of error that still cling to meridian instruments, and especially those connected with flexure and refraction.

The Black Drop.—Prof. J. R. Eastman read a paper on Internal Contacts in Transits of the inferior Planets, in which, after referring to M. André's experiments on the subject, he states that he had independently become convinced, after observation of the transit of Mercury in 1878, that the theory of a necessary "black drop" was fallacious. In 1878 the necessity of the "black drop" phenomena was generally recognized, and many observers of the transit

of Mercury saw it. But Prof. Eastman observes that, so far as he could ascertain, every observer who was accustomed to observe the limbs of the Sun for position (in other words to get, by means of the shade-glasses, best definition of the Sun's limbs with an illumination somewhat less than the eye could easily bear), failed to see any trace of it. The 1882 Transit of Venus confirmed this view. Prof. Eastman saw no "black drop," nor, so far as he was aware, did any observer in any of the eight parties sent out by the United States Transit of Venus Commission. He adds, "the contact was as easily, and I think as accurately, observed as the transit of a star within 8° of the pole under the best condition. The uncertainty of noting the time of the visible contact could not be greater than three tenths of a second." "With a properly arranged telescope and shade-glasses no good observer need have any trouble from any phase of the 'black drop.'"

Jupiter.—Dr. G. W. Hough contributed a paper on Physical Phenomena on the planet Jupiter, in which he reiterates his opinion that the changes on the planet are not sudden but gradual, and that many of the features are permanent. The reports of sudden and great changes are, he thinks, due to the changes in the presentation of the markings caused by the rapid rotation of the planet. He recently observed the equatorial belt "drifting down toward the red spot; but although it partly surrounded it, they did not coalesce, and the spot forced a scallop into the belt—a very curious phenomenon." He had seen a satellite pass over the red spot, although the satellites are not visible on the white part of the disk. He had also compared the shadows of satellites on the disk and on the spot, and both were dark.

SEMI- DIAMETER OF THE MOON *.—Prof. H. M. Paul, formerly assistant at the U. S. Naval Observatory, Washington, gives in Appendix II. of the Washington Observations for 1879 the results of two occultations of the Pleiades group by the Moon, observed by himself to determine the occultation semidiameter of the Moon, and also the corrections to the right ascension, declination, and parallax of the Moon, these being necessarily involved with the semidiameter. The occultations occurred on July 6, 1877, and Sept. 6, 1879, and were observed with the 9.6-inch equatorial of the Washington Observatory. The relative positions adopted for the stars were those of Wolf, with proper motions from comparison with Bessel, and the general proper motion of the group as given by Newcomb. The observations of 1877 were poorly placed for a determination of the correction to the semidiameter; but those of 1879 give a much more reliable result. From the latter (fourteen in all) the resulting correction to Hansen's mean semidiameter ($15' 33''.47$) is $-1''.69 \pm 0''.12$; and the resulting value is therefore, semidiameter = $15' 1''.78 \pm 0''.12$. He gives also the results of Airy's determination from 296 scattered observations from 1830

* 'Science,' Vol. i. No. 20.

to 1860. From the immersions and emersions at the dark limb, the resulting values are larger by $0''.9$ and $0''.5$ than those given by Prof. Paul, and from immersions and emersions at the bright limb, Airy's results are larger by $2''.3$ and $4''.4$. Prof. Paul concludes that the best way to observe the actual occultation at the bright limb is to use as high a magnifying-power as possible, so as to obtain a decided difference of colour between the star and the Moon's limb. Neither set of occultations observed by Prof. Paul gives any evidence of deviation of the Moon's limb from a perfect circle.

PALERMO OBSERVATORY*.—Prof. Riccò, the physical observer at this observatory, observed the Sun on 315 days during 1882, drawing the spots by the method of projection on a scale of $0''.51$ to the Sun's diameter. He also sketched the chromosphere and prominences on 156 days, and on 124 examined regions of special activity for the reversal of the Fraunhofer lines, especially of the coronal line and the *b* lines of magnesium. Jupiter also received a large share of attention. Twenty-four drawings were made of this planet, and the belts and the red spot were measured on 10 nights. The same observer obtained physical and spectroscopic observations of the great September Comet on 55 nights, making 93 sketches and 12 measures of the nucleus; and in conjunction with Prof. Zona observed Comet Wells with the spectroscope and otherwise on 11 nights. Prof. Riccò also observed the Transit of Venus and, with the spectroscope, the eclipse of May 16. In the department of geometrical astronomy, Prof. Zona obtained positions of 22 minor planets, and determined afresh the constants of the ring and filar micrometers of the Equatoreal. The other observations included positions of the great comet, and of comets Wells, Barnard, and observations of the August meteors, of the Transit of Venus, and of the solar eclipse of May 16. The meteorological department embraced the usual subjects.

STONYHURST COLLEGE OBSERVATORY.—The chief astronomical work of this Observatory during 1882, independent of the preparations for observing the Transit of Venus at Madagascar, was the series of observations of solar spots and faculæ, and the measures of the height and extent of the sun's chromosphere and prominences. 227 drawings were made of the entire solar surface, on 186 different days, besides a number of enlarged pictures of remarkable spots. The chromosphere was measured completely on 70 days, and partially on 4 others, and some spot spectra have been examined. The report of the Observatory contains, besides the various meteorological and magnetic reports, an interesting list of the principal magnetic disturbances of the year, and another of the observations of auroræ. The following note as to coincidences between solar outbursts and auroræ is added:—"Even a cursory examination of the daily sunspot drawings made throughout the

* Vierteljahrsschrift der astr. Gesell. 1883, Heft 2.

year shows a striking coincidence of auroral displays with extraordinary manifestations of solar activity. Thus, on the 21st of March, a large group of spots had formed within the last three days. Again, between the 9th of September and the 5th of October, two groups of spots increased immensely in size so as to become visible to the naked eye on the 2nd of October. On the 20th of October a few spots, scarcely larger than pores, gradually joined together, and developed into a large spot of very irregular outline. Another spot became visible on the 23rd, and was large enough on the 26th to be seen without a telescope. This spot, which was at first circular, became considerably elongated on the 29th. Lastly, on the 12th of November a long dark line appeared on the following limb of the sun, the line being an enormous spot seen in perspective, and this was well on the solar disk during the magnificent auroral display and grand magnetic storm of the 17th."

THE SOLAR SPECTRUM.—M. Fievez has just published a chart of the red end of the solar spectrum, between A and C, in continuation of the chart he published some fourteen months ago, and which extended from λ 6600 to λ 4500. As in his former work, the instrument he used was a combination of a Rutherford grating with a Christie direct-vision spectroscope, a combination M. Fievez has found to yield exceedingly good results. In the present research M. Fievez was not able to use the high dispersion he employed in the former one, on account of the comparatively feeble light of this part of the spectrum, and employed the spectrum of the first order from the grating with a single half-prism. The B group as presented by M. Fievez agrees closely with its delineation as given by Prof. Piazzi Smyth, but shows some additional faint lines between the principal lines of the group. These lines are also exhibited in some of the photographs of the spectrum obtained at Harvard College with a Rowland concave grating. M. Fievez's chart shows 360 lines between λ 7700 and λ 6562, and he points out that the origin of only five of these has yet been determined.

DISCOVERY OF COMET 1883 c*.—The discovery is reported, by Prof. Lewis Swift of the Warner Observatory, of a new comet on Sept. 11. Its place was then R.A. $18^h 41^m 56^s$, Dec. $+73^\circ 9'$. It was observed again Sept. $13^h 50^m$ G.M.T., in R.A. $18^h 26^m$ and Dec. $+73^\circ 8'$.

FOUR of the minor planets recently discovered have been named as follows:—No. 225, Henrietta; No. 229, Adelinda; No. 231, Vindobona; and No. 234, Barbara†.

* Dun-Echt Circular, No. 81.

† Circ. zum Berliner Ast. Jahrb. No. 213.

THE BEN-NEVIS OBSERVATORY.—On Sept. 5 Mr. R. T. Omond, for several years past chief assistant to Prof. Tait, was appointed Superintendent of this Observatory. The erection of the buildings is approaching completion, and it is expected that Mr. Omond and the two assistants who are to be appointed will take up their station at the Observatory in readiness for the winter's work in the course of the present month. The work during the coming winter will consist chiefly of eye observations, as the Observatory will not receive its complete instrumental equipment until next year. A telegraph cable is to be laid from the Observatory to Fort William.

RE-DISCOVERY OF COMET PONS, 1812.—The comet of 1812, the return of which to perihelion had been expected to take place about 1884, September, was detected by Mr. Brooks on September 2. MM. Schulhof and Bossert, who had computed a sweeping ephemeris for it, point out that, although its perihelion passage will take place seven months earlier than indicated by their most probable elements, they had found that there was an uncertainty of ± 5 years in its revolution period.

THE RED SPOT ON JUPITER*.—Sig. Riccò, of the Royal Observatory, Palermo, writing to 'Nature' under date Sept. 10, states that he had examined Jupiter since its conjunction, and that the red spot had finally disappeared, the region where it had been now appearing very white.

THE D₃ LINE.—Prof. Palmieri announces the existence in the lava of Vesuvius of a substance the spectrum of which shows the D₃ line.

THE next Course of Lectures upon Astronomy will be delivered in Gresham College on Oct. 16, 17, 18, and 19 at 6 P.M. Subjects—Recent Astronomy; Sidereal Astronomy. They will be of an elementary character.

Elements and Ephemeris of Comet Tempel, 1873 II. (1873-1878) for 1883†. By M. L. SCHULHOF.

1883, Oct. 20.0, Berlin M.T.

M.....	354°	5'	43".5	} Mean eq. and eclip. 1880.0.
π	306	7	4.4	
Ω	121	2	8.4	
i	12	45	17.1	
ϕ	33	32	29.5	
μ	681"	1068		
log a	0.477861			

* 'Nature,' Vol. xxviii. No. 725, p. 487.

† Astron. Nachr. Nr. 2534.

Ephemeris for Berlin Midnight.

1883.		R.A.			Decl.			Log r.	Log Δ.
		h	m	s	°	'	"		
Sept.	29....	15	41	16.88	-14	4	35.4	0.16367	0.27444
	30....	15	44	4.04	14	20	56.9		
Oct.	1....	15	46	52.85	14	37	12.9		
	2....	15	49	42.99	14	53	23.0		
	3....	15	52	34.58	15	9	26.8	0.15891	0.27501
	4....	15	55	27.61	15	25	24.1		
	5....	15	58	22.08	15	41	14.5		
	6....	16	1	17.19	15	56	57.5		
	7....	16	4	15.34	16	12	32.9		
	8....	16	7	14.13	16	28	0.4		
	9....	16	10	14.36	-16	43	19.6	0.15018	0.27616
	21....	16	48	7.65	-19	34	9.9	0.13947	0.27833
	22....	16	51	26.16	19	47	7.7		
	23....	16	54	46.04	19	59	52.0		
	24....	16	58	7.27	20	12	22.4		
	25....	17	1	29.83	20	24	38.4	0.13663	0.27928
	26....	17	4	53.72	20	36	39.7		
	27....	17	8	18.91	20	48	26.0		
	28....	17	11	45.39	20	59	56.8		
	29....	17	15	13.15	-21	11	11.7	0.13419	0.28038

Elements and Ephemeris of Comet Pons-Brooks*.

By MM. SCHULHOF and BOSSERT.

Epoch, 1883, September 30.0.

T.....	1884, Jan. 25.82434,	Berlin M.T.
π.....	93° 20' 47".8	} Mean eq. 1880.0
Ω.....	254 6 15.3	
i.....	74 3 20.4	
e.....	0.9549960	
log q.....	9.8893650	
Period	71.48 years.	

Ephemeris for Berlin Midnight.

1883.		R.A.			Decl.			Log Δ. Brightness.	
		h	m	s	°	'	"		
Oct.	1....	16	27	31.72	+58	47	30.2	0.30913	1.85
	2....	16	28	1.52	58	34	28.8		
	3....	16	28	34.21	58	21	30.3		
	4....	16	29	9.77	58	8	35.0		
	5....	16	29	48.20	57	55	43.1	0.29842	2.04
	6....	16	30	29.47	57	42	54.7		
	7....	16	31	13.54	57	30	10.1		
	8....	16	32	0.41	57	17	29.3		
	9....	16	32	50.08	57	4	52.5	0.28709	2.26
	10....	16	33	42.53	56	52	19.7		
	11....	16	34	37.73	56	39	51.2		
	12....	16	35	35.67	56	27	27.0		
	13....	16	36	36.43	+56	15	7.1	0.27509	2.52

The brightness at the date of discovery is taken as unity.

* Comptes Rendus, Vol. xcvi. No. 12.

Ephemeris of the Great Comet, b 1882.*

By H. KREUTZ.

THE comet does not seem to have been detected during the absence of the Moon at the beginning of September; but the early days of October will afford a better chance of its detection, as it will then rise much earlier, and may be looked for in a dark sky instead of in one strongly lighted by the dawn.

We learn that Dr. B. A. Gould succeeded in observing it at Cordoba so late as June 1.

For Berlin Midnight.									
		R.A.			Dec.			Log Δ.	Log r.
1883.		h	m	s	°	'	"		
Sept. 30	0. . . .	7	33	49.0	— 11	28	6	0.7721	0.7565
Oct.	1. . . .	7	33	53.1	11	33	47		
	2. . . .	7	33	56.5	11	39	29		
	3. . . .	7	33	59.3	11	45	11		
	4. . . .	7	34	1.4	11	50	53	0.7709	0.7595
	5. . . .	7	34	2.8	11	56	35		
	6. . . .	7	34	3.5	12	2	17		
	7. . . .	7	34	3.5	12	7	59		
	8. . . .	7	34	2.9	12	13	41	0.7696	0.7625
	9. . . .	7	34	1.5	12	19	23		
	10. . . .	7	33	59.5	12	25	4		
	11. . . .	7	33	56.7	12	30	45		
	12. . . .	7	33	53.3	— 12	36	26	0.7683	0.7655

Differences of Right Ascension and Declination between Iapetus and the centre of Saturn. By A. MARTH.*

Greenwich Noon.	α—A.	δ—D.	Greenwich Noon.	α—A.	δ—D.
Oct. 1		+37.9 + 64	Oct. 17		+27.6 + 213
2		+39.0 + 79	18		+25.1 + 214
3		+39.8 + 94	19		+22.5 + 213
4		+40.5 + 108	20		+19.8 + 210
5		+40.9 + 121	21		+16.9 + 207
6		+41.0 + 134	22		+13.9 + 202
7		+41.0 + 146	23		+10.8 + 195
8		+40.6 + 158	24		+ 7.6 + 188
9		+40.1 + 168	25		+ 4.4 + 179
10		+39.3 + 178	26		+ 1.2 + 170
11		+38.3 + 186	27		— 2.1 + 159
12		+37.0 + 194	28		— 5.4 + 147
13		+35.5 + 200	29		— 8.6 + 134
14		+33.9 + 205	30		—11.8 + 120
15		+32.0 + 209	31		—14.9 + 105
16		+29.9 + 212			

* Astron. Nachr. No. 2532.

† Monthly Notices, Vol. xliii. No. 8.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS(East).	ENCELADUS.	TETHYS.	DIONE.
Oct. h 4 16·8 ^h 5 15·4 6 14·0 7 12·6 8 11·2 9 9·9 22 14·4 23 13·0 24 11·6 25 10·2 26 8·9 27 7·5	Oct. h 1 13·5 2 22·4 4 7·3 5 16·2 7 1·1 8 10·0 9 18·9 11 3·7 12 12·6 13 21·4 15 6·3 16 15·2 18 0·1 19 9·0 20 17·9 22 2·7 23 11·6 24 20·5 26 5·4 27 14·3 28 23·2 30 8·1 31 17·0	Oct. h 1 15·6 3 12·9 5 10·2 7 7·5 9 4·8 11 2·1 12 23·4 14 20·7 16 18·0 18 15·3 20 12·6 22 9·9 24 7·2 26 4·5 28 1·8 29 23·1 31 20·4	Oct. h 2 4·7 4 22·3 7 16·0 10 9·7 13 3·4 15 21·0 18 14·7 21 8·3 24 2·0 26 19·6 29 13·3
MIMAS(West).		RHEA.	
Oct. h 1 9·6 28 17·4 29 16·0 30 14·6 31 13·2		Oct. h 5 7·5 9 19·9 14 8·2 18 20·6 23 8·9 27 21·2	
HYPERION.		TITAN.	
Sept. 29 22·1 Oct. 21 4·0		Sept. 29 6·5 Oct. 15 4·5 31 2·7	

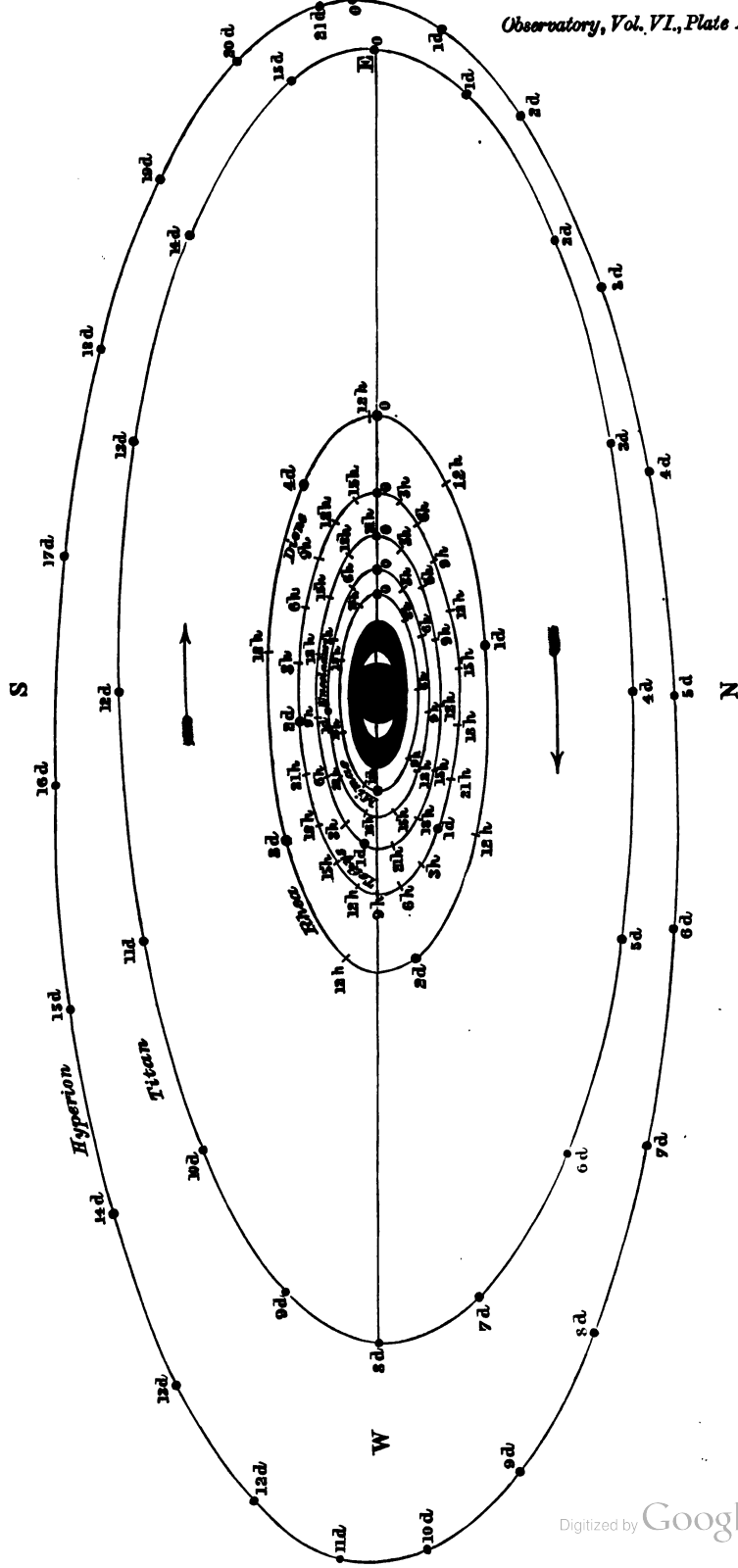
The following table will furnish the means for inferring approximate distances and position-angles of the satellites from the diagram given in Plate I.

Position-angle of Minor Axis of Ring.			Semi-major axis of orbit of Titan.		
Oct.	I	355° 51'	Oct.	I	196".9
	II	355 53		II	200.2
	2I	355 56		2I	203.2
	3I	356 0		3I	205.5

Astronomical Memoranda, 1883, October.

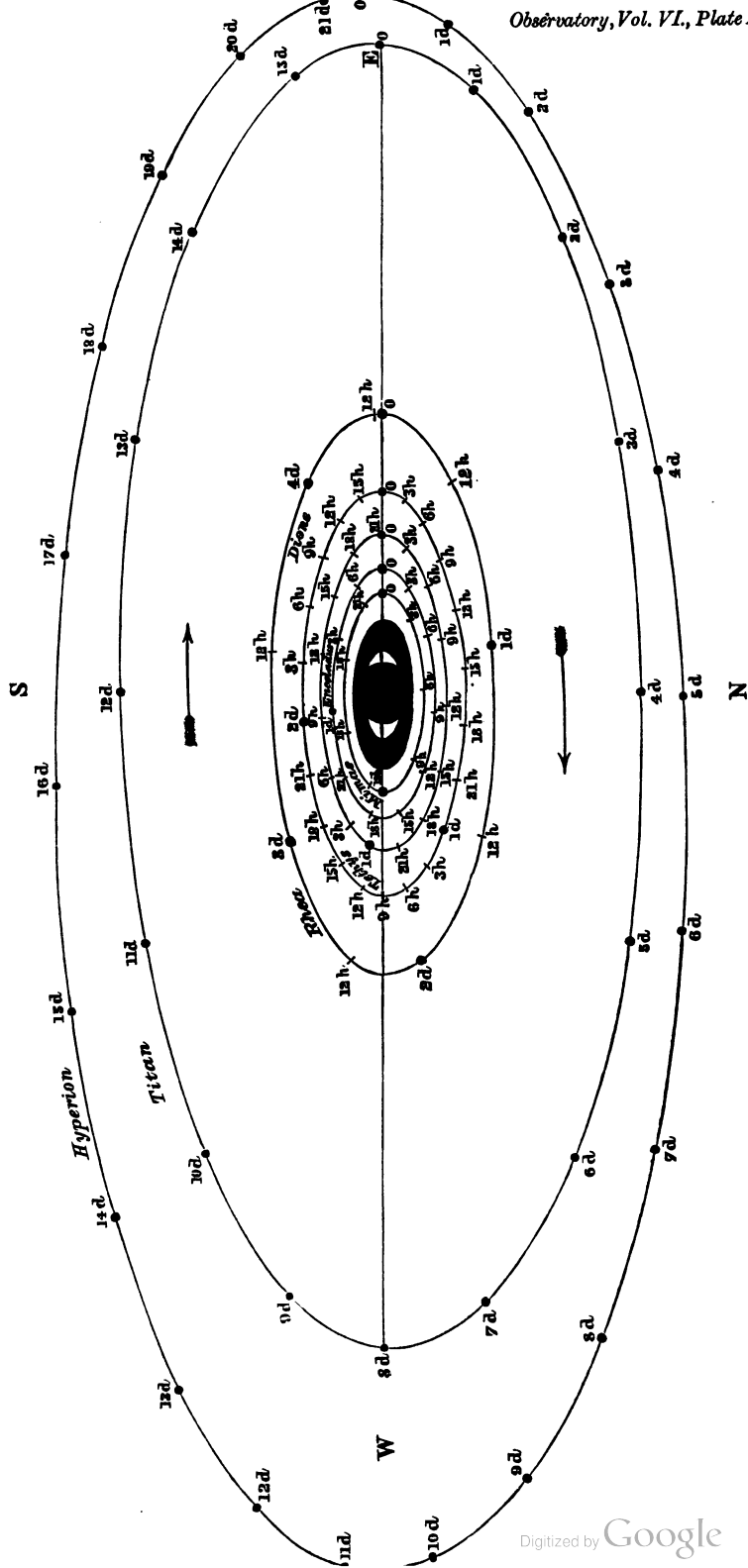
Sun. Oct. 1, sets 5^h 38^m, rises 18^h 4^m; Oct. 11, sets 5^h 15^m, rises 18^h 20^m; Oct. 21, sets 4^h 54^m, rises 18^h 38^m; Oct. 31, sets 4^h 34^m, rises 18^h 55^m.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East elongation.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the satellites. The figures indicate the interval from the time of last East elongation.

Equation of Time:—Sun *before* Clock, Oct. 1, $10^m 16^s$; Oct. 11, $13^m 10^s$; Oct. 21, $15^m 17^s$; Oct. 31, $16^m 16^s$.

Sidereal Time at Mean Noon:—Oct. 1, $12^h 39^m 5$; Oct. 11, $13^h 18^m 9$; Oct. 21, $13^h 58^m 4$; Oct. 31, $14^h 37^m 8$.

Moon.		sets.		sets.		rises.	
		h	m	h	m	h	m
Oct.	1..	5	30	Oct.	11..	9	31
	2..	5	54		12..	10	39
	3..	6	20		13..	11	47
	4..	6	50		14..	12	53
	5..	7	25		15..	13	59
	6..	8	7		16..	15	3
	7..	8	54		17..	16	7
	8..	9	49		18..	17	10
	9..	10	51		19..	18	14
	10..	11	59		20..	sets.	
					31..	4	53

First Quarter, Oct. 8, $22^h 20^m$; Full Moon, Oct. 15, $18^h 45^m$; Last Quarter, Oct. 22, $11^h 19^m$; New Moon, Oct. 30, $11^h 57^m$.

A partial Eclipse of the Moon, partly visible at Greenwich, takes place Oct. 15. Magnitude of the Eclipse (Moon's diameter = 1) 0.277.

First contact with Penumbra $16^h 42^m$, with Shadow $17^h 59^m$.

Last contact with Penumbra $19^h 50^m$, with Shadow $21^h 6^m$.

Mercury is in inferior conjunction with the Sun, Oct. 6, 13^h . He is stationary, Oct. 14, 21^h ; and at his greatest elongation, $18^\circ 16'$ W., Oct. 22, 0^h. He is in conjunction with Venus, Oct. 4, 3^h ; and with γ Virginis, Oct. 20, 7^h .

Venus is near the Sun and unfavourably placed for observation. Diameter:—Oct. 1, $9'' 6$; Oct. 31, $10'' 0$. Illuminated portion of disk, 0.994 on Oct. 15.

Oct. 1, R.A. $12^h 41^m 5$, Dec. $3^\circ 7'$ S., tr. 0^h 2^m , sets $5^h 50^m$
 31, 15 3 5, 16 57 S., 0 26 5 0

Mars is in conjunction with Jupiter, Oct. 19, 7^h ; and in quadrature with the Sun Oct. 31, 19^h . Diameter:—Oct. 1, $7'' 6$; Oct. 31, $9'' 1$. Illuminated portion of disk, 0.887 on Oct. 15.

Oct. 1, R.A. $7^h 40^m 0$, Dec. $22^\circ 19'$ N., tr. $18^h 59^m$, rises $10^h 51^m$
 31, 8 44 1, 19 43 N., 18 5 10 15

Jupiter is in quadrature with the Sun, Oct. 26, 19^h . Diameter:—Oct. 1, $33'' 5$; Oct. 31, $36'' 5$.

Oct. 1, R.A. $8^h 11^m 7$, Dec. $20^\circ 15'$ N., tr. $19^h 30^m$, rises $11^h 35^m$
 31, 8 25 0, 19 37 N., 17 45 9 55

Saturn is retrograding in Taurus.

Oct. 1, R.A. $4^h 34^m 9$, Dec. $20^\circ 1'$ N., tr. $15^h 53^m$, rises $8^h 0^m$
 31, 4 29 7, 19 47 N., 13 50 5 58

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Oct. 6	44'' 14	19'' 28	29'' 35	12'' 82	17'' 6
Oct. 26	45 42	19 77	30 20	13 15	18 0

The south side of the rings is visible, the elevation of the Earth above their plane being $25^{\circ} 54'$ on Oct. 6, and $25^{\circ} 48'$ on Oct. 26, and of the Sun $25^{\circ} 31'$ and $25^{\circ} 37'$.

Neptune is in Aries.

Oct. 1, R.A. $3^h 15^m \cdot 1$, Dec. $16^{\circ} 12' N.$, tr. $14^h 33^m$, rises $7^h 4^m$
 31, 3 12 3, 16 0 N., 12 32 5 4

An Annular Eclipse of the Sun, invisible at Greenwich, takes place on Oct. 30. It will be visible as a partial eclipse on either side of Behring's Straits.

Phenomena.

G. M. T.		G. M. T.	
h	m	h	m
Oct. 3	16 56 J. iii. Ec. D.	Oct. 18	16 52 δ^8 Tauri
5	14 42 J. ii. Ec. D.		Oct. R. 322° .
	15 41 J. iv. Ec. D.	21	12 1 λ Geminorum
	16 20 J. i. Ec. D.		Oct. D. 95° .
6	14 45 J. i. Tr. I.	13	2 λ Geminorum
	17 4 J. i. Tr. E.		Oct. R. 256° .
7	14 20 J. i. Oc. R.	14	35 J. i. Ec. D.
	14 49 J. ii. Tr. E.	17	8 J. ii. Tr. I.
	15 21 J. iii. Tr. E.	22	13 4 J. i. Tr. I.
12	17 18 J. ii. Ec. D.	13	27 J. iv. Ec. R.
13	16 40 J. i. Tr. I.	15	23 J. i. Tr. E.
14	12 1 J. iv. Tr. I.	23	12 37 J. i. Oc. R.
	12 42 J. i. Ec. D.	13	52 κ Canceri
	14 33 J. ii. Tr. I.		Oct. D. 102° .
	15 56 J. iii. Tr. I.	14	43 J. ii. Oc. R.
	16 15 J. iv. Tr. E.	14	50 κ Canceri
	17 26 J. ii. Tr. E.		Oct. R. 230° .
15	13 29 J. i. Tr. E.	25	13 29 J. iii. Oc. R.
16	12 4 J. ii. Oc. R.	28	16 28 J. i. Ec. D.
18	7 48 B. A. C. 1272	29	14 58 J. i. Tr. I.
	Oct. D. 75° .	17	17 J. i. Tr. E.
	8 35 B. A. C. 1272	30	11 50 J. ii. Ec. D.
	Oct. R. 307° .	14	30 J. i. Oc. R.
16	5 δ^8 Tauri	17	19 J. ii. Oc. R.
	Oct. D. 46° .	31	11 46 J. i. Tr. E.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

ERRATA IN No. 77.

Page 276, line 7, for *semidiameter* read *diameter*.

„ 280, line 35, for *Phil. Trans.* read *Phil. Mag.*



THE BIRTH-PLACE OF JEREMIAH HORROX, AT OTTERSPOOL, NEAR TOXTETH, LIVERPOOL.

[*From the PALATINE NOTE-BOOK.*]

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 79.

NOVEMBER 1.

1883.

Meeting of the Liverpool Astronomical Society.

THE third session of this Society was opened on Wednesday, September 26, under extremely favourable circumstances, both so far as the attendance was concerned and in the selection of the speakers. The chair was occupied by the President (Mr. B. C. Johnson, F.R.A.S.), who said it was customary, on the first evening of a session, for the President to deliver an opening address; they had had the good fortune, however, to secure the attendance of Professor E. C. Pickering, who had been kind enough to take this duty upon himself. He was sure they would be anxious to hear the remarks of Professor Pickering, so he would, without trespassing upon their patience, at once introduce him.

Professor Pickering, who was received with applause, said:—"Mr. President,—The encouragement of astronomy, which your Society has in view, is an important work, and it gives me great pleasure to address you. I would speak to-night more particularly about the work to be done in connection with stellar light and stellar variation. In this branch of astronomy valuable work can be done with the unassisted eye or with a small telescope or opera-glass. First of all, as regards star-magnitudes, the earliest catalogue, that of Ptolemy, dates nearly 2000 years ago. In it the determinations of brightness are shown by numbers. The faintest star has the number *six* affixed, and the brightest *one*. But as there are many stars of the same magnitude and yet differing in brightness, the Greek letters μ or ϵ were affixed, signifying the words *μετζων* or *ἐλάσσων* (greater or less). Various copies of this work are in existence, and valuable work might be done if some gentleman, who possessed classical and astronomical skill, were to compile the differences in these copies. The variation between the MSS. is very considerable. Bailey found that the assigned magnitudes or positions varied in three fourths of the total number of stars. The great authority at present is a ninth-century MS. at Paris in the national library. A century ago Sir W. Herschel commenced his photometric measures, his method being to speak

of a star as brighter or fainter than some other star. He designated small differences of magnitude by means of a period, a comma, or a dash and their combinations. Four catalogues are given in the 'Philosophical Transactions.' They attracted little attention, however, as there were no absolute determinations of magnitude. At Harvard College all the stars down to the sixth magnitude inclusive have been photometrically observed, and by means of these measures we have been able to represent in numbers the estimates of Herschel. But these four catalogues refer only to parts of the sky. Only two thirds are there given. Whilst in London I was allowed, by the kindness of the possessors of the original manuscripts of Sir W. Herschel, to inspect them, and amongst them I found two more catalogues which had remained unpublished. A note was attached, in Miss Caroline Herschel's handwriting, stating that they were ready for the press. They had been left to Sir John Herschel, and, probably through pressure of work, he did not publish them, or else did not realize their value. I was allowed to make a copy of these two catalogues, and have forwarded them to Harvard. (Great applause.) No similar work of equal accuracy has appeared till within the last ten years. Half a century later Argelander, the greatest observer, perhaps, who has ever lived, discovered the same method independently, and for a long time, so slow was intercommunication in those days, was not aware of Sir W. Herschel's earlier work. Argelander's method is one of the most precise. The principal difference between his and Herschel's is that the former used a notation while he used numbers. If the difference was small he called it 'one grade,' if distinctly visible 'two grades,' and so on. There are 25,000 observations of his in the 'Bonn Observations.' The great difficulty at present, which any one studying the magnitude of the stars as they were in former years has to encounter, is that the various observations of them are scattered far and wide in various periodicals. The results of these observations we hope to collect in one volume. While at Bonn I was allowed to see Argelander's observations. There were some 3000 or 4000 referring to the variable stars. By the kindness of Professor Schönfeld I was allowed to copy them. As many of them were written in the dark, probably with the eye at the telescope, it was a work of some difficulty to decipher them. The number of unpublished observations is very great. No piece of work is accessible to all till it is printed. At present I find, speaking from memory, that there must be nearly 170,000 unpublished observations, and not more than 40,000 published. It is greatly to be regretted that they should thus be left to accumulate, and perhaps in the end never get into print. Here let me point out a source of error liable to occur in Argelander's and Herschel's method. This is due to the effect of one star's position with regard to another. Two stars, apparently equal, show a difference when placed one above the other. Through the varying positions of the stars as presented to the observer, have arisen many of the cases

of suspected variation. The suspected variable stars amount to no less than 1500, according to the list compiled at Harvard; but many of them may have been suspected of variation through the cause just mentioned. Valuable contributions would be made to our knowledge if it could be determined why this apparent variation, due to position, exists. The question is worthy investigation, and I would suggest the following methods:—(1) To look at the stars near the pole at different seasons of the year, or, as can be done more conveniently in your more northern latitude, early in the evening and late in the morning. (2) By inverting the positions by means of a looking-glass. (3) By watching stars in the zenith, lying down on the ground, and then turning round and looking up at them in a different position. The brightest stars are less important than the fainter ones, because sufficiently observed elsewhere. The stars below the sixth magnitude are very little observed. Valuable work might be done by determining the magnitude of the stars visible in an opera-glass, in the manner of Argelander and Herschel, and especially I would recommend those stars near the pole. Another matter of interest would be to take some of the brighter clusters, and arrange the stars in the order of brightness. Another piece of work no less valuable might be done in determining the brightness of Uranus and Neptune and of the Asteroids, some of which are probably variable. We know that one of the satellites of Saturn (Iapetus) varies very greatly in light; and it has been suggested, with great probability of truth, that one side is capable of reflecting more light than the other, and so we have a means of determining the time of rotation through observations of its variation. The Asteroids may vary in the same way, and the value of the observations lies in the fact that we shall thus have a probability of finding the time of rotation on their axes. Similar observations of Uranus and Neptune might be made; but probably the differences would not be large. Not much value can be assigned to the times of rotation of these planets as hitherto determined. But this method of watching the variation may be valuable, as determining the times of rotation of those bodies on which we are not able to see markings. Thus it is impossible to measure the diameter of the stars, as we never have been, and in all probability never shall be able to observe their disks. The diameter of the brightest star does exceed a few hundredths of a second, corresponding to as many feet at a distance of 4000 miles. But it may be possible to tell with precision the size of bodies whose disks are invisible. For instance, in 1877 and 1879 we made determinations of the brightness of the satellites of Mars, and found that, if of the same material as Mars, they could only have a diameter of six or seven miles. We can tell their relative diameter by their measures with greater accuracy than has yet been attained for Mars itself. Again, Vesta might be compared with various stars throughout the year, by this means a list of comparison stars would be obtained stretching right round the heavens. Another advantage would be that as the asteroid

varies with its distance from the Earth, we can compute what the variation should be, and so correct our eye estimation. Another field is one in which good work is being done by your Society, viz. the observation of suspected variable stars. The greater part of these are visible to the naked eye, and are thus easily observed, so that the greater part of the work may be done without any instrumental help. A great point would be gained even if the results were purely negative, as these stars could then be removed from the lists of suspected variables. Yet another point is the observation of the variation of absorption. This consists merely of watching stars and estimating the brightness of stars on the horizon and in the zenith. All that is necessary is to compare carefully a star on the horizon with a star near the zenith. These observations should be made in various places. In America, the pure sky of Italy has become proverbial, and I had hoped to have reduced this poetical saying to a mathematical figure; but, with the exception of one opportunity, I was unable to make the determinations I had intended doing while travelling from place to place. This exception was on the Rigi, with a field-glass; some stars near the horizon were compared with stars in the zenith, and I hope hereafter to reduce the results thus obtained. But the subject commends itself to travellers, and I hope any member of your Society who has the chance may attempt this kind of work." (Prolonged applause.)

The President said a communication had been received from Mr. E. F. Sawyer, of Cambridgeport, U. S. A., on some observations he had made on the variable star R Coronæ, from 1881 to the present time. He suggested that this star should be closely watched by astronomers.

The Rev. T. E. Espin, B.A., gave a brief account of the work done at the Society's observatory. The whole of the sky had, he said, been divided into zones, and the work of watching for variable stars apportioned to ten observers. The situation of each observer had also been taken into consideration, and, for the most important zones, were as far as possible apart, in the hope that should it not be clear at one it may be at the other. Comparison stars have already been issued for the first six hours of right ascension, containing somewhat over 100 stars.

Eight candidates were proposed for election, and the proceedings closed with a vote of thanks to the lecturer.

Jeremiah Horrox.*

THE family of Jeremiah Horrox derived its name from Horrox, or Horrocks Fold (otherwise, in early records, Horrocks Forth), near Rumworth, N.W. of Bolton. The same place-name also occurs near Standish. The best known member of this Lancashire family

* Abridged from the 'Palatine Note-book,' 1882 December, by the kind permission of the author.

in the xvijth century was Alexander Horrocks, the Puritan Vicar of Deane, afterwards of West Haughton, who died before 1650, and who was spoken of by the Royalist soldiers as "that old rogue that preaches in his grey cloak." John Cotton, the Puritan (1585-1652), married for his first wife Mrs. Elizabeth Horrocks, described as "sister to Mr. James Horrocks, an excellent minister in Lancashire" (Brooks's 'Puritans,' iij. 153),—a mistake, most likely, for Alexander, of Deane. Another relative was Christopher Horrockes, Fuller, of Horrockes Hall, Bolton-le-Moors, who, with his family, went into New England with Mr. Cotton, leaving, however, an only son, Thomas, at St. John's College, Cambridge (adm. a sizar, April 1631). The latter, refusing an invitation "to a great school at Manchester," was licensed by the Assembly of Divines, and appointed by the Earl of Manchester, Sept. 1644, minister of Great Cressingham, Norfolk, and afterwards settled at Malden, Essex. Another contemporary of the astronomer is met with in John Horrocks of Pendle and Colne, who is said to have been an able divine.

Jeremiah Horrox was born at Toxteth, a small village near Otterspool, at a house (once connected with the ancient Park of Toxteth) called the Lower Lodge, near the junction of the Otterspool with the Mersey. This house here depicted*, from a water-colour drawing at the Liverpool Library, was pulled down in 1863, when the Garston and Liverpool Railway was made. The site is near the Otterspool Station. The Otter's pool may still be seen to the left in passing from Sefton Park to that station.

He was, it is said, the son of a small farmer, of Puritan stock, who may perhaps be identified with one "William Horrockes, of Toxteth Parke," whose will and inventory are at Chester. The will is unsigned, and there is no date of execution; but it was made in all probability before April 1619, though the endorsement says 1646. Richard Mather, who is mentioned in it, was the celebrated Puritan preacher of Toxteth and Prescott, and Incumbent of Dingle Chapel until 1633, when he was silenced, and who in 1635 emigrated to America. The will is therefore anterior to the latter date. The year is important, because there is a William Horrocks met with at a later date. William "Horrockes" took the Protestation with his Liverpool neighbours on 2 February, 1641-42. The same individual, Wm. "Horroks," is the fifth on a list of jurors sworn at a Sessions of the Peace at Liverpool, 27 June, 1642; and he is found once more on a list of the Liverpool burgesses, 13 Feb., 1644.

Another important member of the Toxteth Park Horrockses was James, a watchmaker. When his brother Christopher Horrocks, of the foulds, co. Lanc., yeoman, made his will, 16 Aug., 1631, James was indebted to him £160. "James Horrocks, my brother," was appointed sole executor. This will was proved 5 Feb., 1638-39. Another James Horrocks took the Protestation of

* Plate II.

1641-42 at Liverpool, with the other grown-up male members of his family. The registers of Liverpool, which begin late, record the burial, 5 Sept., 1663, of Clare Harrocks, widow; and the marriage, 28 April, 1677, of Margaret Horrocks and William Carter, by licence.

The astronomer is generally said to have been born in 1619; but the account in the Underhill MSS. seems inclined to place it two years earlier. Dr. Wallis was certain, from information he had, that Jeremiah, at the time of his death in 1641, was 22 years of age, or at most in the beginning of his 23rd year. He had a younger brother, Jonas, of whom a few notices have survived, who became infected with Jeremiah's enthusiasm in the science of the stars. "Jonah Horrokes" likewise appears on the list of the Liverpool Protestors.

When very young, we are told, Jeremiah was placed under a country schoolmaster, whom he soon surpassed. Sir J. A. Picton conjectures that this instructor might be Richard Mather, of Toxteth, just alluded to; but this could scarcely be the case, as Mather left his school in 1618 to enter the ministry. Jeremiah, brought up, as we see, in a strict Puritan home, with his Puritan neighbours the Aspinwalls, was well tutored in the rudiments of Latin and Greek; but he manifested an extraordinary liking for mathematics and astronomy, which may have taken its rise in the workshop of James Horrocks the watchmaker, whose business included the art of dialling. The pursuit of the study of astronomy was beset with difficulty owing to the scarcity of books, of which only a few Latin treatises could be had. By chance the youth fell in with a tract by Henry Gellibrand (Professor of Astronomy in Gresham College from 1626 to 1636), in which Philip Lansberg, the Flemish astronomer, was spoken of in terms of praise; and Lansberg's tables for some time formed Horrox's text-book, and he studied them and held them in high regard. His familiarity with the tidal river near his birthplace first took his attention to the tides, which was one of the subjects he had under study at the close of his short life (Op. p. 334). His astronomical life is related in his own words in the *Prolegomena* to his '*Opera Posthuma*,' p. 2, where he speaks of the way in which he overcame the difficulties of his position. In nervous and enthusiastic language he describes the great delight which he experienced in thinking of the fame of those excellent masters in the science, Tycho and Kepler, whom he would seek at least to emulate in his aspirations. He deemed that nothing could be more glorious than to contemplate the manifold wisdom of his Creator amidst so great a profusion of works, and to behold the pleasing variety of the celestial motions, the eclipses of Sun and Moon, and other conjunctions, no longer with the unmeaning gaze of vulgar admiration, but in order to know their causes, and to feed upon their beauty by a closer inspection of their mechanism.

On 18 May, 1632, he entered as a sizar in Emmanuel College,

Cambridge, then under the mastership of William Sancroft. From Bishop Bennet's Register of the Students we derive this date; and it is added that he was born at Toxteth, and was a very curious astronomer (IV. Rep. Hist. MSS., p. 417). At the University he was as badly off in respect to instructors in mathematics and astronomy as in Lancashire; but books were more accessible, and he was brought into the society of those to whom these pursuits had charms. Dr. John Worthington, of Manchester, who entered Emmanuel College a few months earlier, viz. on 30 March, 1632, became well acquainted with Horrox; and in a letter to Hartlib, 28 April, 1659, Worthington says:—"He lived at Toxteth Park, near Liverpool, in Lancashire; was sometime of Emmanuel College, Cambridge, admitted the same year I was" ('Diary,' p. 61). Dr. John Wallis, the mathematician and the editor of Horrox's '*Opera Posthuma*,' 1672, entered as a pensioner in the same College in 1633; Samuel Foster had also belonged to it; and Cudworth and Seth Ward were contemporaries with them.

His stay at Cambridge lasted three years. He does not appear to have taken the first degree in arts, for his name is not in Cole's lists; but he had acquired the knowledge requisite for entering the Church. Moreover he continued to give his mind to the study of astronomy. He also paid attention to the cultivation of the poetic faculty, and learned to write verse with ease. There are several poems embodied in his writings, including one to his telescope. His reference ('*Opera Post.*' p. 16) to Dr. George Hakewill's '*Apology or Declaration of the Power and Providence of God in the Government of the World*,' with other passages of the like eloquence, show that he was ever under the influence of reverential feelings, and was in sympathy with the spirit of the proverb that declares that the undevout astronomer is mad. His astronomical education left him singularly free from the teachings of the astrologers (see Whatton, p. 113). He had learned to shape rude contrivances to aid him in scanning the heavens; but relied most of all upon his own observation, for he said that no one who had eyes could be said to want astronomical instruments (*cf.* *Op. Post.* pp. 252, 306; and Whatton, p. 139). He tells us that before his practical observations began, he had calculated the Ephemerides of several years from Lansberg's tables; and this indefatigable task seems to reach into his college days (*cf.* *Op. Post.* p. 295). The lives and works of Tycho Brahe and Kepler he also studied, and he subsequently defended the first-named against the cavils of Hortensius, of Delph.

The library of Trinity College, Cambridge, contains a book which connects itself with Horrox's college days, being a copy of Lansberg's tables, purchased by himself at the University in 1635, and presented to the library by Augustus de Morgan, 11 April, 1841. It is thus entitled:—

"*Philippi Lansbergii Tabulæ Motuum cœlestium perpetuæ:*

item novæ et genuinæ Motuum cœlestium theoricæ; et astronomicarum observationum Thesaurus.

"Middleburgi Zelandiæ, apud Zachariam Romanum. 1632. fol."

This precious relic contains the following inscription in three lines, on the recto of page 1 (half-title): *Jeremiah Horrocks hunc librum possidet | pro quo dedit + * ꝑꝑt. Cantabrigiæ. | 1635 incipiente.* After Horrox's death the volume passed into possession of the Towneley family, and has the book-plate of Richard Towneley, 1702. Horrox, as we gather from the inscription, began the study of the folio in 1635; and his writings show that he constantly used it. More interesting still is an alphabetical list of writers in astronomy, also in his own writing, on the verso of the last leaf, showing what authors were available for a young student to take up.

From Cambridge Horrox returned to Toxteth. His published astronomical observations and letters serve to fix his movements from this time with some precision. His first observation at Toxteth is dated 7 June, 1635 (p. 365); and his first letter to his friend, William Crabtree, of Broughton, is dated 21 June (p. 247). At page 4 of his *Prolegomena* we get the date of his personal acquaintance with his accomplished correspondent: "Anno Christi 1636. ingeniosus juvenis *W. Crabtrius* (qui prope Mancestriam non procul a me degebat, & annos jam aliquot his Studiis impenderat) tandem mihi notus factus est & familiaris." The 'Liverpool Repository' of 1826, following Smither's 'History of Liverpool,' stated that Horrox became acquainted with Crabtree at Emmanuel College, and the statement has elsewhere been repeated. But the above facts do not bear out this assertion; and, besides, Worthington in the letter just quoted, after naming Horrox, speaks of "one Mr. Crabtree, a Lancashire man, and his great correspondent in these studies,"—which are not the terms in which the writer would have spoken of a fellow-collegian. Crabtree had, we see, for some years been a diligent student of the movements of the heavenly bodies, and had satisfied himself of the inaccuracy of the tables of Lansberg. After some correspondence he persuaded Horrox to put no further trust in that author; and at length Horrox gave way. By making common observations the two friends proved that Lansberg's hypotheses "would never agree with the heavens for all times, as he confidently boasts; no, nor scarce for any one whole year together." Henceforth the Rudolphine tables (so called because Kepler calculated them for King Rudolph of Bohemia) occupied the chief place in their libraries; and they satisfied themselves of the true places of the planets in the heavens, and so could accurately foretell conjunctions &c. One of Horrox's dissertations was a defence of the Keplerian astronomy against the hypotheses of Lansberg, "which he proves to be inconsistent among themselves, not agreeable with former observations exactly made" (Sherburne).

Horrox's letters &c. indicate that he remained at Toxteth, with one or two short interruptions, up to 1 June, 1639 (p. 323). In this interval thirty-one of his letters to Crabtree are named in the quarto; but unfortunately Dr. Wallis the editor not only abridged them or made extracts from them, but turned them into Latin, ut ab exteris intelligantur (*cf.* pp. 239, 243). The abridgment is much to be regretted, as we thus lose many details that might serve to reveal the writer's character and illustrate his career.

We get many hints about Horrox's troubles with instruments or his contrivances for want of them. At page 252 he describes what Wallis calls a "pinnacidium." At page 309 he notes that he bought a "tuba" for 2s. 6d. And at page 247 is a general complaint of his ill equipment. The telescope, invented by Lipperhay the Hollander, was described to Galileo about the year 1609, and he first turned it to good use. In time Horrox, by the aid of friends, had perfected an instrument of such power as to show the spots on the sun; and it was with this telescope that he observed the transit of Venus.

In June 1638 he visited Crabtree (p. 308). On 22 May, 1639, he tells us that he observed most clearly an eclipse of the Sun through his telescope in a darkened chamber; and he describes it at length (pp. 387-89). In his letter dated 1 June, 1639, given in Latin by Wallis, he wrote to his friend Crabtree: "Sequente Septimanâ sum commigraturus *Hoolam*; exigua est illa villa, 5 milliariibus citra *Prestonam*," *i.e.* "On the 7th of the following month I am removing to Hoole, a small hamlet five [8] miles this side Preston." His first observation at Hoole is dated 25 June, 1639 (p. 390); and his first letter from Hoole is dated 8 July (p. 324).

He had accepted duty at the Church of St. Michael's, Hoole, to which he may at this time have been ordained. Hoole or Much Hoole, then a chapelry, is eight miles south-west of Preston, in the parish of Croston, and about 20 miles north of Liverpool. The village, which in 1641 contained 235 persons of both sexes over the age of 18, engaged in agriculture, was situated on a piece of low-lying land, having a large morass on the east, and the waters of Marton Mere and the river Douglas or Astland on the south. Hoole is 2 miles north of the point where this stream meets the Ribble, and the estuary of the latter is in view. The ancient chapel had been rebuilt in 1628, when it was endowed by Mr. John Stones, of Hoole and London, and other inhabitants, the ground being given by Sir Peter Leigh and Sir Thomas Bartron. Under this endowment the curate's stipend was £40 per annum, and the right of nomination lay with Mr. Stones. In 1641 Hoole was made into a separate parish with the consent of the Rev. James Hiet, the Rector of Croston, the right of patronage being confirmed to Mr. Andrew Stones, who about the close of that year presented Mr. Robert Fogg to the living. When the survey of 1650 was made Mr. Samuel Jones was the incumbent, his stipend

being £53. We allude to these details to show that Horrox was not the possessor of the benefice, but merely the curate or *locumtenens*; otherwise his own references to his narrow means &c. would be unexplainable.

This solitary and obscure spot, situated on an extensive plain not far from the sea, had an advantage in that it left the astronomer free to pursue his studies without much interruption. The amusements of the neighbourhood were chiefly hunting and hawking, and Gascoigne, a young country gentleman of Yorkshire, found the same lack of studious pursuits amongst his neighbours. The house marked out by tradition as the dwelling of Horrox was Carr House, about half a mile south of the church, in the township of Bretherton, a quaint and substantial structure of brick, with projecting porch and gable facing the south. The late Rev. R. Brickel, Rector of Hoole, pleased himself in thinking that the centre room on the first floor, with the recessed window over the porch, was that in which Horrox saw the transit. The house is about one mile north of the river Douglas.

It was here that Horrox, from tables drawn up by himself, calculated the positions of the planets; and it was while so engaged, late in the autumn of 1639, that he had his first intimation, about the beginning of October, of the real time of the conjunction of the Sun and Venus. The knowledge he had acquired induced him, "in expectation of so grand a spectacle," to observe the heavens with increased attention.

In his letter to Crabtree, dated Hoole, 26 October, 1639, Horrox, after a month's interval, explains (Op. Post. p. 331) that the reason why he now writes was that he might advise him of a remarkable conjunction of the Sun and Venus, which would take place on the 24 November,—a conjunction which had never happened for many years back, and would not occur again in their time:—

"I beg therefore most earnestly (obnixè) that you will use the telescope with attention, and make whatever observation you can, especially of the diameter of Venus, which indeed according to Kepler is 7', according to Lansberg 11', but according to my proportion scarcely greater than 1'. And if this letter reaches you in good time I beg that you will acquaint Mr. [Samuel] Foster [of Gresham College, London] of the same fact (which, I doubt not, will be very pleasing to him); for it may happen that in many places the sky will be cloudy, (the earth, the sun, Venus, Mercury, and Jupiter being in the same right line), so that it will be desirable that observations should be made in several places in reference to so important a phenomenon. According to Kepler, the true conjunction will be visible at 8h. 8m. a.m. at Manchester, on the 24th November, 1639; i. e. at 9h. 3m. at Uraniburg, lat. 14' 10" south [Tycho Brahe's observatory on the island of Hveen]; but according to my correction will be seen 5h. 57m. at Manchester, with 10' south latitude, the latitude decreasing 39" in one hour.

But since a slight change of numbers in Kepler's Tables (which indeed might be admitted notwithstanding other observations which I know), would much alter the time of conjunction and the quantity of the (planet's) latitude; it will be best to look for it throughout the day, even from the evening before and up to the following morning, if it is not seen in the meanwhile. But I quite think that it will take place on the 24th day [Julian time *].

In the 'Venus in Sole Visa' he also says that hereupon he immediately wrote to his very learned friend William Crabtree—one, says he, who has few superiors in mathematical learning, inviting him to be present at the Uranian banquet. Horrox also acquainted his younger brother, Jonas, who then resided at Liverpool, hoping that he would exert himself on the occasion *. Other persons, he explains, he could not communicate with, having had so short a notice of the phenomenon.

The critical day, which was not again to happen for 122 years, was on Sunday. Horrox describes minutely his preparations for the event, and more than once refers to the smallness of the apartment which he used as an observatory. The plan he adopted to observe it was that of Gassendi—to let the Sun's image through a telescope into a darkened room, rather than through a naked aperture, as Kepler recommended, because the latter method wanted a large room. The largest figure of the Sun that Horrox could obtain was in a circle 6 in. across. He did not expect the conjunction before three o'clock: yet he watched through the greater part of Saturday, which was cloudy. "I watched carefully on the 24th (Sunday), which was a clearer day, from sunrise up to nine o'clock; also from a little before ten until noon, and at one in the afternoon. During the intervening hours I was called away to matters of higher importance, which for these pursuits I could not under any circumstances properly neglect (*ad majora avocabar quæ utique ob hæc πᾶρεργα negligi non decuit*)."

Morning prayers in the chapel took him away in the first interval, for matins were said then earlier in the day than now, giving more propriety to the phrase in the collect, "who hast safely brought us to the beginning of this day." The afternoon prayers or catechizing, with sermon, took up the other two hours. "At 15m. past 3 in the afternoon," he continues, "when I was again at liberty to continue my observations, the clouds, as if by divine interposition, were entirely dispersed, and I was once more invited to my willing task. I then beheld a most agreeable spectacle, the object of so many wishes,—a spot of unusual magnitude and of a perfectly circular shape, which had already entered upon the Sun's disk on the left, so that the limbs of the Sun and Venus perfectly coincided, forming an

* "This indeed he did, but it was in vain; for on the 24th the sky was overcast, and he was unable to see any thing, although he watched very carefully. He examined the Sun again on the following day, which was somewhat clearer, but with no better success, Venus having already completed her transit."

angle of contact. Not doubting that this was the shadow of the planet, I immediately applied myself sedulously to observe it”*.

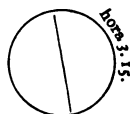
In his “Observations” in *Op. Post.* (p. 393), under date of November 24, 1639, we have the account put in a more concise form, thus:—

“Observavi per Telescopium Conjunctionem Solis & Veneris nobilissimam, in obscura camera.

“Diameter Veneris fuit scr. 1' 10", qualium Sol habet 30', certè non major.

“Venus intravit discum Solis ad sinistram [*lege dextram*] gr. 62. 30' (certè inter 60 & 65) à vertice; fuitque eadem inclinatio constans usque ad occasum Solis. Hoc intus in radio; contrarium in celo apparuit. Fuitque Venus inferior centro Solis ad sinistram: Sic

<i>Horolog.</i>	<i>Distantia Centrorum.</i>	
3. 15 p	Scrupula	14' 25"
3. 35 p	qualium	13 30
3. 45 p	Sol 30	13 0
3. 50 p	Occasus Solis apparens."	



One of the most striking passages in Horrox's treatise is that in which he apostrophizes America, where the transits are seen in their full continuance (Hevelius, p. 118). In poetic imagery he reproaches Venus for wasting her charms on those barbarous lands. He relates how eagerly he had followed her path here; and he concludes with the confident assurance that posterity would view the same magnificent spectacle.

There are two other letters to his friend Crabtree, written from Hoole, and dated 2 March and 20 April, 1640. In the latter, which refers to his want of leisure, he says that he wants, if it could be had, Gassendi's work, ‘*Mercurius in Sole visus et Venus invisa Parisiis an. 1631*’ (it was published at Paris in 1632, fol.), before he issued his own treatise (p. 333; and *cf.* p. 382). The Hoole observations extend up to 29 June (p. 398). He then removed to Toxteth, probably on account of ill-health; and the last letters to his friend are dated respectively 18 and 30 July, 12 September, 3 October, and 12 and 19 December, 1640. In that of October he speaks of the uncertainty of his affairs. In that of 12 December (pp. 335–38), in answer to some discovery his friend had made, he laments that the state of his affairs is less fixed, and his daily troubles, calling him away elsewhere, did not allow of his being able to rejoice at hand with him (Crabtree), “not, at least,

* This passage is quoted from the Greenwich MS. in the ‘*Letters of Scientific Men*,’ ii. p. 111; and it varies somewhat from the text of Hevelius. Costard has the following passage in his ‘*Astronomy*,’ page 184:—In the year 1639, November the 24th, at Hool, a little town about fifteen miles to the North of *Liverpool*, in *Lancashire*, HORROX, a young clergyman there, saw, the first person of any that ever lived, *Venus in the Sun*.”

to testify my joy as I wish, and to congratulate you. Certainly if I were not kept by a great necessity, by which I am either unwillingly detained at home or compelled to journeys less pleasing, I would long since have hastened to you at Broughton, that I might more fully know what new matters you are giving your mind to." For three months past he says he has made no observations, but he hoped that if he remained another year at Toxteth he would find out many secrets. "As for yourself, continue your observations; and I will prepare to enter into them again as soon as I have settled my business (*mea negotia*). In some respects I have altered my '*Venus in Sole Visa*,' but have not yet had leisure to transcribe it; and if it were done I do not yet know how to look after an edition of it. I beg of you to seek your bookseller*, who may guide us in this matter." He finally hints at a visit to Crabtree about Christmas. Horrox's last letter, 19 December (of which Wallis has only given three lines), says that he could at length fix Monday, 4 January, 1641, as the day of his visit, adding, "*nisi quid præter solitum impediât, me tunc expectes.*"

At the back of this letter Crabtree wrote "Mr. Jeremiah Horrox's Letters to me in the years 1638, 1639, 1640, up to the day of his death very suddenly, on the morning of 3 January; the day before he had arranged to come to me. Thus God puts an end to all worldly affairs. I have lost alas! my most dear Horrox. *Hinc illæ lachrimæ! Irreparable loss!*"

He was interred within the walls of the ancient chapel at Toxteth Park; but no memorial marked the spot. In the year 1826 Mr. Moses Holden, of Preston, interested himself in erecting at St. Michael's a tablet in black and grey marble, which is placed over the north door of the new church to the memory of this precursor of Newton.

At the end of 1875 a marble scroll was placed on the pedestal of the monument of John Conduitt (nephew of Sir Isaac Newton, who adopted Horrox's theory of lunar motion), at the west end of the nave of Westminster Abbey, bearing this inscription, from the pen of Dean Stanley:—

*"Ad majora avocatus
quæ ob hæc parerga negligi non decuit."*

In Memory of
JEREMIAH HORROCKS,
Curate of Hoole in Lancashire,
Who died on the 3d of January, 1641, in or near his
22d year;
Having in so short a life
Detected the long inequality in the mean motion of
Jupiter and Saturn;
Discovered the orbit of the Moon to be an ellipse;
Determined the motion of the lunar apse,
Suggested the physical cause of its revolution;
And predicted from his own observations the

* Perhaps Thomas Smith, who had come to Manchester in 1624. He published some Civil War tracts and sermons, and died 1653-54.

Transit of Venus,
Which was seen by himself and his friend
William Crabtree
On Sunday, the 24th November (o.s.) 1639 :
This Tablet, facing the Monument of Newton,
Was raised after the lapse of more than two centuries
December 9, 1874.

The position chosen for this memorial is most admirable, for Horrox's writings contained the suggestions of the discoveries of Newton ; and the inscription is not less happily conceived, for Horrox is as closely connected with Venus as Kepler is with Mars. In a conversation with the writer a few years ago the late Dean Stanley seemed pleased in relating that he had been the means of placing two memorials in the Abbey to two famous Lancashire men,—Jeremiah Horrox and Major-General Worsley.

JOHN E. BAILEY.

Spectroscopic Notes.*

FOR the past few months I have been examining the spectra of sun-spots with great care, and with an instrument of high dispersion.

The spectroscope employed consists of a comet-seeker of five inches aperture and about forty-eight inches focal length, used both as collimator and view-telescope after Littrow's method, the slit and diagonal eyepiece being as close together as it is convenient to place them. A small spot of black paper, about three-tenths of an inch in diameter, is cemented to the centre of the object-glass (as suggested by my colleague, Professor Brackett, in a note published in this Journal, July 1882), and entirely destroys the internal reflections which would otherwise most seriously interfere with vision.

The dispersion is obtained by one of Professor Rowland's magnificent gratings on a speculum-metal plane, with a ruled surface three and one-half inches by five ; 14,000 lines to the inch. The slit and eyepiece of the telescope are so placed that the line joining them is parallel to the lines of the ruling. An instrument of this sort is incomparably more convenient than one in which the collimator and view-telescope are separate, though of course, on account of the inclination of the visual rays to the axis of the object-glass, there is a little aberration, and the *maximissimum* of definition is not quite reached. There is no difficulty, however, in seeing easily the duplicity of the *U's*, *E's*, and other similar tests, with the instrument thus arranged. The spectroscope is mounted upon a strong plank, stiffly braced, and this is attached by powerful ring-clamps to the tail-piece of the twenty-three-inch equatoreal of the Halsted Observatory, so that the image of the Sun falls directly upon the slit.

The detailed examination of the spot-spectra has been thus far

* 'American Journal of Science and Art,' Nov. 1883. Communicated by the Author.

confined mainly to a few limited regions in the neighbourhood of C, D, and b .

With the high dispersive power employed, the widening and "winging" of the heavier lines of the spectrum is not well seen—not nearly so well as with a single-prism spectroscope. All diffuse shadings disappear much in the same way as the naked-eye markings on the Moon's face vanish in a powerful telescope, to be replaced by others more minute but not less interesting. In a few spots, however, the broadening of the D's and the reversal and occasional "lumping" of C has been noticeable even with this high dispersion. But the most striking result is that in certain regions the spectrum of the spot-nucleus, instead of appearing as a mere continuous shade, crossed here and there by markings dark and light, is resolved into a countless number of lines, exceedingly fine and closely packed, interrupted frequently between E and F' (and occasionally below E) by lines as bright as the spectrum outside the spot. These bright lines, so far as the eye can determine, may be either real lines *superposed*, or merely vacancies left in the shading of fine dark lines, since they are not sensibly brighter than the ordinary background of the surrounding spectrum.

The darker and more intense the spot, the more distinctly the fine lines come out, both the bright and the dark; and, so far as I have been able to make out yet, there is no difference as regards these fine lines between one spot and another. I have never yet seen any evidence of displacement in them due to motion, no "lumpiness" nor want of smoothness in them.

When seeing is at the best, and everything favourable, close attention enables one to trace nearly all these lines out beyond the spot and its penumbra. But they are so exceedingly faint on the Sun's general surface that usually they cannot be detected outside the spot-spectrum. This resolution of the spot-spectrum into a congeries of fine lines is most easily made out in the green and blue. Near D, and below it, it is much more difficult to see, and I am not even quite sure that this structure still exists in the regions around C and below it. Here, in the red, even with the highest dispersion and under the most favourable circumstances of vision, the spot-spectrum appears simply as a continuous shade, crossed here and there by widened and darkened lines, which, however, are very few and far between as compared with the number of such lines in the higher regions.

Of course the resolution of the spot-spectrum into lines tends to indicate that the absorption which darkens the centre of a sun-spot is produced, not by granules of solid or liquid matter, but by matter in the gaseous form; and it becomes interesting to inquire what substances are capable of producing such a spectrum, and under what conditions. As to the fineness and number of the lines, it may be noted that in the region included between b_1 and b_2 , the single lines appear to be each about half as wide as the components of b_n , and are separated by an interval about one-third as

great. The whole number between b_1 and b_4 must be over a hundred, though they are of course very difficult to count with accuracy. They are a little wider in the middle of the spot-spectrum, in fact spindle-shaped, running out into extremely fine threads where they pass into the penumbra, and in my instrument they seem to be a little more hardly and sharply defined on the upper (more refrangible) edge than on the lower.

The bright lines, of which there are six between b_1 and b_4 , are generally about as wide as the interspace between the components of b_3 . They are sharply defined at both edges, and no brighter at the centre than at the edge, a fact which rather bears in favour of the idea that they are merely interruptions in the dark line series, and not really superposed bright lines. Just above b_4 (at λ 5162.3) there is a very conspicuous one, which is also noticeable enough in the ordinary solar spectrum. Attention has indeed been frequently called to it long since by other observers. Below E these bright lines are rare. Higher up the spectrum, between F and G, they become very numerous.

I have also made a considerable number of observations upon prominences with the nine and one-half inch equatorial and its own spectrocope. There have been lately numerous very fine exhibitions, especially in connection with the spots. The number of lines reversed in the spectrum of the chromosphere has at times been very great, far exceeding the number observed and catalogued in 1872, but I have not been able to detect a single *new* one below C, though the two mentioned in my catalogue have been seen almost continuously. On two occasions—July 31st and August 1st—a new line above H (λ 3884 $\pm$ 2) was conspicuously visible for an hour or two each time, during a specially vigorous eruption of the prominences associated with the great spot which was then just passing off the limb. This line was seen easily without the aid of any fluorescent eyepiece, and I am satisfied that on a photographic plate it would have been more brilliant than either H or K. I could not determine its position within one or two units on account of the difficulty of identifying the numberless fine lines around it.

With the widened slit it showed clearly the form of the lower part of the prominence, but not the upper. It was almost precisely imitated by two new lines at λ 4092 and 4026; and the catalogue-lines 4077 and 3990 resembled it also. On the other hand, h , H, and K showed the higher parts of the prominence as well as the lower, while the lines at 4045 and 3970 were exceedingly fine and smooth, without knottiness or structure.

On August 1st, at 2^h 58^m local time (= 7^h 57^m Greenwich time), the intensity of the chromosphere spectrum was very remarkable, the bright lines more vivid and numerous than I remember ever to have seen them before. Between this time and 3.12 a prominence was shot up in fragments of flame to an elevation of over 120,000 miles. It will be interesting to learn whether any corresponding magnetic twitch appears on the magnetometer records.

Princeton, N. J., 1883, Sept. 10.

C. A. YOUNG.

Diameter, Form, and Surface-markings of Uranus, from Observations at Princeton, N. J., with the 23-inch Equatorial.

PROF. SAFARIK's article upon the form of Uranus (which has been translated in the 'Observatory') called my attention to the favourable circumstances presented this year, and led to the observations of which the results are here given. Bad weather and engagements connected with college duties prevented as early a beginning of the work as desirable; and between May 18 and June 19th only 14 complete series of measures could be made, on account of interruptions from the weather and other causes. The measures were mostly made with bright wires upon a dark field, the micrometer being provided with Burnham's illumination; but 5 of the 14 were made without illumination in the twilight.

Reduced to distance 19.1826 [log. 1.28291], they give:—

Polar diameter	3".974 $\pm 0".030$
Equatorial diameter	4".280 $\pm 0".022$
Ellipticity	$\frac{1}{4}$

These measures are considerably larger than Schiaparelli's results (published in the 'Observatory' last Sept.), but agree very nearly with the measures of Mädler in 1843, who obtained polar diameter 3".874, equatorial 4".304.

It is probable that my figures are too large, and there certainly is a systematic difference between the twilight measures and the others (the former being smaller), which gives rather a larger probable error to the final result; but neither of these circumstances affects the value of the ellipticity deduced. There is, however, some doubt as to the ellipticity from the fact that the observations were begun so late that it was impossible to obtain the desired symmetry in the relation of the two measured diameters to the vertical, and so a further correction may be necessary on account of the personal equation between my measures of vertical and horizontal lines. But the accordance of so many different observers leaves it beyond a reasonable doubt that the planet's disk is considerably flattened, its equator lying sensibly in the same plane with the satellite-orbits.

Whenever the seeing was good two belts were always faintly but unmistakably visible on each side of the equator, much like the belts of Saturn. On one or two occasions other belts were suspected near the poles. On two nights the general trend of the belts seemed to make an angle of 20° or 30° with the major axis of the Satellite-orbits, and on one occasion there seemed to be a little projection from the edge of one of the belts toward the pole. It was not found possible to make out the rotation-period of the planet from the data so far gathered, but it is hoped that another year better success may be met with in this respect.

Princeton, 1883, Sept. 14.

C. A. YOUNG.

Spectroscopic Observation of γ Cassiopeia.

ON August 22nd my friend M. Eugen de Gothard informed me by letter that he had seen the *bright C* line in the spectrum of γ Cassiopeia; but, on account of the unfavourable atmospheric conditions, he was not sure that the line might not be merely a subjective phenomenon caused by contrast with a dark band more refrangible than C.

I observed the star on the same evening with the following result:—In a spectroscope of one train of prisms (three prisms of the Zöllner eyepiece spectroscope), and with a cylindrical lens, the line appeared very faint; but applying two trains of prisms, without any cylindrical lens, I saw it as an exceedingly bright knot on the narrow thread of the spectrum. After longer observation I noticed the F line also, though it was much fainter than C.

I tried to make a comparison with a Geissler hydrogen tube. The observation was made with a little star-spectroscope (containing one prism of 60° , collimator, slit, telescope, and comparison prism) which M. Eugen de Gothard had kindly constructed for me. I did not employ any cylindrical lens, but arranged the instrument so that the narrow star-spectrum appeared projected on the faint hydrogen spectrum. The coincidence of the lines was perfect. The slit needed to be opened to a considerable width; therefore the lines of the spectrum from the Geissler tube were somewhat broad. The above-mentioned knot of light, C, lay just in the middle of the red band.

A comparison with F was impossible, the bright line in the star being very faint, and therefore invisible beneath the bright F line of the comparison spectrum. I remarked a strong absorption-band in the star-spectrum slightly more refrangible than C; its edge nearer the violet end of the spectrum was exceedingly diffused, the edge nearer the red was sharply bounded by C. On the less refrangible side of C I saw another broad line, the reality of which, however, I could not guarantee, as it might be only a contrast phenomenon.

On August 26th I observed the spectrum at the astrophysical observatory, Herény, with the $10\frac{1}{4}$ -inch Browning reflector and a Zöllner eyepiece spectroscope. The air was exceedingly clear and steady. The red line was exceedingly bright; D₃ and F were well defined; and later, when the star reached a greater altitude, I succeeded in seeing the hydrogen line *h*, as has been already mentioned by M. de Gothard, in the 'Astronomische Nachrichten,' No. 2539. The dark absorption band, more refrangible than C, was very distinctly defined, and I felt assured that the line somewhat less refrangible than C was not an effect of contrast.

On August 27th 11^h M.V.T. I observed the same star with the 27-inch refractor of the Imperial Vienna Observatory, which my friend Prof. Edmund Weiss kindly placed at my disposal for spectroscopic observations. In this observation I used, however, my own eyepiece and spectroscope, viz. a Merz orthoscopic eyepiece of

2-inch focal length, and a Zöllner eyepiece spectroscope, also by Merz, with two sets of prisms. Using no cylindrical lens, C appeared a knot of light of dazzling brilliance, D, F, and *h* were fainter; with the cylindrical lens the spectrum was still bright enough to have enabled me to take some measures easily; but I had no apparatus for this purpose. The C line was, as I have already mentioned, the brightest. The broad absorption band, a little more refrangible than C, was exceedingly strong; towards the red it is bounded by C, towards the violet it is shaded. The line is not a simple one: it consists of a broad line in the immediate neighbourhood of C, with a fainter shading in which I distinctly saw three dark stripes.

The absorption band on the other side of C had undoubtedly a real existence; this is the more certain since it does not lie *close* to C, a circumstance which can only be ascertained by means of so large an instrument as the Vienna 27-inch refractor. It seems that this band is sharply bounded towards the red, but diffused on the opposite side.

After a while I could positively assert the presence of the (dark) D and the *b* groups, though they were very faint. Sometimes I thought I saw some very faint lines in the green and blue, but was uncertain whether this might not be an illusion caused by the fluctuation of the spectrum.

There was also a line more refrangible than *h*, and, like the former ones, sharply bounded towards the red, but fading away and resembling a nebulous shadow towards the violet.

O'-Gyalla Observatory,
1883, Sept. 30.

NICOLAS DE KONKOLY.

Spectroscopic Observations of Comet Pons-Brooks.

I OBSERVED the spectrum of this faint comet on the 27th September, 10^h 40^m, under not very satisfactory atmospheric conditions, with a spectroscope * by Professor H. C. Vogel, of Potsdam.

The spectrum was very faint, and appeared as a diffused nebulous spot of light. After looking in the spectroscope for a long time in an entirely dark room, I saw three bright bands in the spectrum. The bands were all extremely faint and diffused on both sides, the slit being opened rather wide. The second band (from the red end) was the brightest, the third band was the next brightest, and the least refrangible band near red portion of the spectrum was the faintest. All three bands ended in a faint point; they were of unequal lengths, the brightest band being the longest, the least refrangible next, and the band towards the more refrangible end of the spectrum the shortest.

There were moments in which, for one or two seconds at a time,

* Konkoly, 'Anleitung zur Anstellung astronomischer Beobachtungen,' etc. J. Viewig & Son, Braunschweig, p. 705.

the bands were very clear and bright, and at these times they seemed to be much shorter than ordinarily, I should say but one third the length—a singular phenomenon, which I have never seen on any other occasion.

O'-Gyalla Observatory,
1883, Sept. 30.

NICOLAS DE KONKOLY.

Spectroscopic Survey of the Northern Heavens *.

SUCH a survey has been begun at the Astrophysical Observatory at Potsdam, the first thoroughly systematic work of the kind since Secchi, and after him D'Arrest, spectroscopically examined a large number of fixed stars. Professor Vogel intends to prepare a complete spectroscopic star catalogue, and a good portion of the work has already been accomplished. To prepare such a catalogue, says Vogel, is a duty which the present generation owes to posterity. The changes taking place in the stars are of especial interest to us and are of importance to science; and although it may be conjectured that changes in the spectra will show themselves soonest in those stars which have proceeded further in their development, that is, in the red stars, yet this cannot be positively affirmed *a priori*. Equally with those wonderful spectra of the red stars, which so enchant the eye of the observer, will changes take place in the course of time in the simple spectra of the white and yellow stars, so that investigations of as large a number of star-spectra as possible, without limiting them to particular classes of stars, are absolutely necessary for future researches.

Dr. Dunér, in Lund, has contemporaneously begun, on the same plan, the observation of stars round the North Pole, so that the work is begun on two sides.

Professor Vogel has published part of his investigations—the zone between -1° and $+20^{\circ}$ declination; the second part, from $+20^{\circ}$ to $+40^{\circ}$, will quickly follow. The stars have been completely surveyed down to the magnitude $7\frac{1}{2}$, and a large number of smaller ones were examined along with them; altogether they number nearly 12,000, and on an average 3 stars were found in a field of view $19'$ in diameter, which were examined together.

Vogel has, as is known, arranged all stars spectroscopically in three chief divisions, of which the first is again subdivided into three, and the others into two subdivisions. Of these, no stars belonging to Class I c (in which the hydrogen line and the line D appear bright) nor to II b (in which, besides dark lines and bands, several bright lines also appear) are found in the published zone.

Of the 4051 stars particularized in the Catalogue, there are 349 whose spectrum could not be surely made out. The remainder are divided among Vogel's classes as follows:—Class I a 2155, Class I b 10, Class II a 1240, Class III a 288, Class III b 9 stars.

* 'Sirius,' Band xv. Heft 9.

Among the brighter stars with very beautifully distinct spectra that therefore can be seen with weaker powers may be mentioned:—

In Class I *a*. γ Geminorum, α Leonis, β Leonis, α Ophiuci,
 α Aquilæ, α Pegasi.

In Class II *a*. α Tauri, α Bootis, α Serpentis, β Ophiuci, α Ophiuci,
 γ Aquilæ, ϵ Pegasi.

In Class III *a*. α Ceti, α Orionis, δ Virginis, α Herculis.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Divisions in Saturn's Rings.

SIR,—

Dr. Meyer's researches in regard to the divisions in Saturn's rings* must be interesting to all astronomers. My own discussion of the same subject, however, permit me to say, was not restricted to Cassini's division, but included also that of Encke. Below I take the liberty of transcribing the conclusion of a paper read before the American Philosophical Society, October 6, 1871, and published in their Proceedings, vol. xii. p. 163:—

"The rings of Saturn, formerly supposed to be solid and continuous, are now regarded as consisting of an indefinite number of extremely small satellites. They are, in short, a compact cluster of secondary asteroids, analogous to the primary zone between Mars and Jupiter. In the latter, it is true, a large proportion of the primitive matter has collected in distinct planetary masses, while a similar result has been prevented in the Saturnian rings by their proximity to the central body. In one respect, however, we observe a striking correspondence. It has been shown that several positions occur in the asteroid zone where planetary periods would have simple rotations of commensurability with the period of Jupiter, and that portions of the original ring occupying these positions would be liable to great disturbance. Now, the ring of Saturn is evidently subject to like perturbation by the nearest satellites. Hence gaps or chasms, analogous to those in the zone of asteroids, ought also to be found in the secondary ring. It has accordingly been noticed that Cassini's division occurs precisely where the periods of satellites would be commensurable with those of the four members of the system immediately exterior†.

"But astronomers have sometimes seen the ring of Saturn apparently separated by several black lines into concentric annuli. At other times, however, no such divisions could be detected. The fact, therefore, of the permanence of these gaps is extremely

* 'Observatory,' Sept. 1883, p. 276.

† 'Meteoric Astronomy,' chap. xii.

doubtful, except in the case of a division of the exterior bright ring. This has been frequently seen by eminent astronomers; and it is probable, though, not absolutely certain, that it is never entirely closed. Most observers agree in placing it outside of the middle of the exterior ring. Let us now inquire whether any simple relation of commensurability obtains between the periods of satellites revolving at the distance of this outermost gap and those of Mimas, Enceladus, Tethys, and Dione.

$\frac{1}{4}$	of the period of Mimas	= 12 ^h 56 ^m
$\frac{2}{7}$	„ Tethys	= 12 57
$\frac{1}{5}$	„ Dione	= 13 8
$\frac{2}{5}$	„ Enceladus	= 13 9

The interior radius of the outer ring	= 1'9963
The radius of a circle bisecting the outer ring	= 2'1209
The distance of a satellite whose period is 12 ^h 56 ^m	= 2'1473
The distance of a satellite whose period is 13 ^h 9 ^m	= 2'1510
The exterior radius of the outer ring	= 2'2456

“It is thus seen that just beyond the middle of the outer ring where the division is actually found, another gap occurs in which the periodic times of satellites would be commensurable with those of Mimas, Enceladus, Tethys, and Dione.”

As I am not aware that this paper on the Formation and Primitive Structure of the Solar System was ever elsewhere published, it may not be improper to give the foregoing extract in the pages of the ‘Observatory.’ In conclusion, it affords me no little gratification to find the proposed explanation of Cassini’s and Encke’s divisions in Saturn’s rings accepted, and the principle still further extended by so distinguished an astronomer as Dr. Meyer.

Bloomington, Ind., U.S.A.,
September, 1883.

DANIEL KIRKWOOD.

Nomenclature of Comets.

SIR,—

My letter to you of the 10th of August was intended merely to obviate any possible confusion with regard to the three comets of short period which owe their first discovery to Tempel. Mr. Denning’s letter of the 8th of September touches upon the general question of the nomenclature of periodical comets, which the progress of discovery is rendering a somewhat difficult one, accentuated, it appears to me, by the recent re-discovery of the comet of 1812. Mr. Denning justly remarks that calling a comet by the name of its discoverer is intended not so much to confer honour on the observer who was fortunate or patient enough to see it first, as for a convenient means of referring to it. When one astronomer, however, has discovered several comets, this method loses great part of its convenience. Nevertheless by adding the year of discovery in the case of a comet moving in a parabola, or in an ellipse so elongated as to be nearly a parabola, the nomencla-

ture is still usually sufficiently distinctive, although even in these the addition of a number is sometimes necessary, as in the case of the two comets discovered by Coggia in the year 1874. But it is with regard to periodical comets of short or comparatively short period that the principal difficulty occurs. As Mr. Denning says, it has been usual to call such a one by the name of the astronomer who discovered it at the return at which its period was definitively determined. This rule has, however, in some cases been departed from. Halley's comet, of course, forms an altogether special one, being the first in which the return of a comet was definitely predicted at all. In the case of Encke's comet, too, the circumstances were felt to be exceptionally remarkable. At the return of 1818-19 it was discovered by Pons, and Encke himself always called it Pons's comet. But from his complete investigation of its orbit, proving that the period of revolution was little more than three years, and that it was identical with comets seen in 1786, 1795, and 1805, the general voice of astronomers decided that it should be as inseparably connected with the name of Encke as that of Halley had been with his since its return in 1682. The other case is that of the comet first discovered by Pons in 1819, and afterwards by Winnecke in 1858; and I agree with Mr. Denning that the designation Pons's comet is preferable to Winnecke's by which it is sometimes called.

But what shall we say with regard to this new return of another periodical comet, also discovered by Pons, and the orbit of which was determined by Encke? Whilst I do not wish in any way to detract from Mr. Brooks's merit in detecting it so early at this return, I presume the designation Pons-Brooks's comet is understood to be only provisional. According to rule, it should be Pons's comet; but that would not be sufficiently distinctive even with the addition "periodical;" its permanent name must therefore be "Pons's long-period comet," or "Pons's periodical comet of 1812." It so happens that it was the only comet of any kind seen in that year.

Yours faithfully,

W. T. LYNN.

Blackheath,
1883, Oct. 10.

Early English Eclipses.

SIR,—

On page 40 of his 'Cycle,' vol. i., Admiral Smyth gives a long list of early English mathematical and philosophical writers, reaching from Oliver, of Malmesbury, A.D. 1060, to Edward Wright, A.D. 1599. Amongst them are such as Louis of Caerleon, 1360, "Calculations of Eclipses." Can any reader mention any records of eclipses seen in our own country that are given by any of these writers?

Faithfully yours,

S. J. JOHNSON.

Melplash, Bridport,
1883, Oct. 1.

The August Meteors, 1883.

SIR,—

As no one else seems to have sent you any account of the Perseid meteor-shower this year, I will try to do so, as it is advisable that some regular record should be kept of each recurrence of the phenomenon.

On the 9th I saw the first of them, it having been overcast previously. In $2\frac{1}{2}$ hours, I saw 85 meteors, of which 61 were Perseids, but at $12^h 30^m$ I left off watching.

The next night was fine for only about 2 hours. I was out from 10^h until $1^h 30^m$, and counted 130 meteors, 113 being Perseids. The best hour produced 46 Perseids. There was some difference in the colour of the meteors. The larger ones were generally yellow and orange, but one very bright one, passing close to Vega at $10^h 45^m$, was of a very similar tint to the star.

At $11^h 35^m$ I noted one about equal to Sirius, near β Ursæ, which was green, ending with a flash, at which point a spot of light was left for 10 seconds.

The 11th is generally the finest night as far as weather goes, and has always been my "lucky" night. I watched the whole night, 5 hours, and saw 186 meteors, of which 157 were Perseids, the highest number in an hour being 43; so that this year's display cannot be reckoned a fine one by any means.

At $9^h 54^m$ a very fine orange meteor with a greenish streak shot from Cassiopeia to just below Lacerta, ending with a rather brighter outburst; this part of its track was marked by an almost detached part of the streak, which lasted 15^s , and gradually bent upwards. At about 12^h , a brilliant yellow one fell from ϵ Boötis, leaving a wide yellow streak for 12^s . In the following 5^m I counted 10 meteors, but it was too good to last. A small meteor noted at $1^h 30^m$ was almost stationary and left a distinctly green streak. Its radiant must have been at about $43^\circ + 56^\circ$. The general radiant seemed to be at $46^\circ + 57^\circ$, but a few from near the great cluster $29^\circ + 56^\circ$.

At $1^h 50^m$ there was an orange-red meteor, leaving a beaded streak; the next large one at $1^h 55^m$, about equal to Jupiter, near β Ceti, was pale yellow with a wide yellow streak for 10^s . I have mentioned these larger ones, not because they were very brilliant, but to show the great difference in colour, in this one shower, both in the nuclei and the streaks.

It is much to be hoped that something may be done this autumn to find whether any connection exists between the bright, slow-moving meteors from west of the Pleiades in October and the Taurids from east of them in November. These are extremely interesting on account of the very brilliant meteors, in which this shower seems so rich. As the Moon will be absent at the critical time, something may be done this season. What requires to be determined seems to be, whether the centre moves from Musca to Taurus, or whether it breaks out in quite a fresh place in November.

I should be glad to hear what really took place amongst Jupiter's satellites on the morning of Oct. 15. I was not out in time, not getting to the telescope until 4^h 5^m, when Sat. IV. was quite away from the planet, instead of being in front of it, until 4^h 15^m. I rather doubted if the satellites were really all invisible at any time.

Great Baddow,
1882, October 22.

Yours truly,
H. CORDER.

Motion of Jupiter's South Equatorial Dark Belt.

SIR,—

In your notes of papers read before the American Association for the Advancement of Science ('Observatory,' p. 306), Dr. G. W. Hough is said to have recently observed the equatorial belt of Jupiter "drifting down toward the red spot." This is so contrary to my own seeings, and I believe also to those of other observers, that I trust you will allow me to make a few remarks on the subject.

In 1880-81, a broad dark band was visible all round the planet, situate at a considerable distance south of the equator, but separated from the red spot by a considerable interval. This belt is shown in all the drawings of that time, and has been very generally designated the "south equatorial dark belt." In the opposition of 1881-82, a curious curved streak was noticed by many observers, which commencing at the "south equatorial dark belt," just under the following end of the red spot, extended south-east until nearly opposite the end of the red spot, and then bent to the east, and ran for some distance parallel to the equatorial bands. At the beginning of the apparition of 1882-83, this curved streak was seen to have enormously increased in breadth and plainness, so as to form a very striking feature, and gradually it extended right round the planet almost up to the preceding end of the red spot, when turning sharply to the north-east it joined the "south equatorial dark belt," and thus formed, with the similar shaped elbow at the following end of the red spot, a rather deep bay, which, roughly following the contour of the spot, extended nearly as far south as the centre of that object. This new belt continued to increase in breadth and plainness, until it fully equalled the "south equatorial dark belt," and formed with it a curious double belt, the components of which, as at the present time, were of about equal breadth, plainness, and redness of hue, and were separated from each other by a narrow well-defined space, except just at the red spot, where the two ends of the southern component bent northwards and joined the northern one.

In all this, it will be observed, there is nothing to indicate that the equatorial belt drifted down towards the red spot; but merely the formation of a new belt immediately south of, and parallel to, the "south equatorial dark belt." The curved arm of the new band just preceding the red spot was thought to have receded a little further from this belt, the only signs of a southward motion

given by my observations, towards the end of the apparition; but in this I am not supported by some drawings of Mr. W. F. Denning, published on p. 49 of the French serial '*L'Astronomie*.'

I have been particular in drawing attention to this matter, because it seems to be of considerable importance. For if an observer were at the present time to measure the position of the south edge of the equatoreal band, he would undoubtedly select the south edge of this new belt, and then comparing his measures with those obtained in 1880, say, of the south edge of the old "south equatoreal dark belt," would doubtless infer, especially if supported by such a statement as Prof. Hough is said to have made, that the equatoreal band, or its southern edge at least, had moved considerably southwards; whilst such supposed motion would really be caused by the formation of this new belt, immediately south of the original "south equatoreal dark belt." Yours faithfully,

8 Denbigh Street,
1883, October 20.

A. STANLEY WILLIAMS.

Satellites of Jupiter.

SIR,—

The time given in the '*Nautical Almanac*' for the Oc. R. of No. II., on October 16, was 12^h 4^m. Some little time in advance I began watching Jupiter's limb, and at length saw what I took to be II. On looking at the time I found it was just 12^h 4^m, without taking odd seconds of time into account. Though a careful watch was kept during 20 minutes, II. was not so much as glimpsed again till 12^h 24^m, when it reappeared and remained perfectly visible. I attributed the 20^m of invisibility to be due to a disagreeable boiling at the planet's limb, and thought no more of the matter. I have since doubted whether that will explain total invisibility for so long a time following on the first sight of the satellite, seeing that the conditions as regarded unsteadiness of the air were from first to last neither better nor worse, and that the satellites have been picked up on the disk under worse conditions. On looking at the positions of the two it seems just possible that the shadow of I. should fall somewhere in the direction of II., and I have wondered whether II. got eclipsed in the shadow of I. at the time.

On October the 12th there was much fog about, which interfered with continuous observation and accurate vision. At midnight on that occasion, I. and II. were seen slightly separated. In three quarters of an hour they showed as one body, elongated with power of 200. In another quarter of an hour the fog became impenetrable, but just at the last I. had apparently entirely occulted II., as no perceptible elongation was visible with a power of 120. The telescope used on both occasions was a 3-inch Dollond.

I am, Sir,

Yours faithfully,

P. F. DUXE.

Hendon, 1883, Oct. 22.

Bright Shooting-Star.

SIR,—

At 9^h 33^m in the moonlit sky of Oct. 17 I observed a bright shooting-star falling amongst the stars of Hercules. It moved somewhat slowly, in a direction not much inclined from the vertical, along a path of 28°, beginning at α 259°, δ 48½° +, and ending at α 241½°, δ 24° +. It traversed the first 20° of its course very slowly, and was little brighter than a 1st mag. star, then it became nearly extinct, but immediately rekindled and gave a succession of three outbursts, the central one of which was the brightest, and there was a distinct flash accompanying it, so that the light must have been quite equal to that of Venus. Simultaneously with these outbursts, the flight of the meteor acquired a sudden impetus, and left short streaks in the several points of their occurrence.

The date corresponds with the October meteoric epoch, but the direction and appearance of this shooting-star prove that it could hardly have belonged to the *Orionids*. Tracing its path backwards it is found to converge upon the region between Taurus and Aries. The date seems too early for the *Taurids*, and it is probable that the radiant-point lay in Aries, in which case this meteor must have been a member of a very rich shower, observed here on October 15, 1879, from the point, α 31°, δ 9° +. Of 127 shooting-stars which I recorded on a watch of 11 hours on that night no less than 21 belonged to this particular stream*, and their visible features as I remember them were similar in some respects to the meteor now referred to.

Yours faithfully,

W. F. DENNING.

Bristol, 1883, Oct. 18.

Saturn's Ring.

SIR,—

An observation of Saturn, made at Naples by Francisco Fontana in 1633, presents a curious analogy with Ball's famous one of October 13, 1665, which has been the subject of so much discussion in your pages. The plate representing what the Neapolitan astronomer *supposed* that he saw, is thus described at p. 130 of "*Novae Coelestium, terrestriumque rerum Observationes*. A Francisco Fontana specillis à se inventis, et ad summam perfectionem (?) perductis, editae" (Napoli, 1646):—

"Anno 1633. Saturni figura non perfectè spherica, sed *supra*, ac *infra* *concavatas habens partes* (à quibus duae stellae quasi semilunae oriebantur, totum Saturni corpus formantes, maioris longitudinis, ac latitudinis) deprehendebatur."

The woodcut in question exhibits the body of the planet as oblong, and relatively insignificant in size, but guarded by and partially concealing two enormous circular appendages, one on each side. Some similar arrangement, it is tolerably evident, Sir R. Moray designed to indicate by his enigmatical suggestion of "two bodies of

* See 'Observatory,' Dec. 1879, p. 247, where these observations are described.

a Circular Figure embracing the Diske" of Saturn. The "notches" or "hollownesses" in Ball's drawing (described with singular precision in the italicised words above) would thus find a ready explanation as due to the intersection of two crossed rings.

I am, Sir,

Your obedient servant,

London, 1883, Oct. 11.

A. M. CLERKE.

NOTES.

PHOTOGRAPHING THE CORONA WITHOUT AN ECLIPSE.—In a paper read before the British Association, Dr. Huggins gives an account of his more recent experiments in coronal photography. The photographs referred to in his first paper read before the Royal Society 1882, Dec. 21, were obtained with a Newtonian reflector by Short. Since then Miss Lassell has lent Dr. Huggins a seven-foot Newtonian telescope made by the late Mr. Lassell. No secondary reflector is used nor is the mirror tilted, but the open end of the tube is fitted with a mahogany cover in which are two circular holes, three and a quarter inches in diameter. Through one of these the light is admitted, and the framework for carrying the sensitive plates is fitted over the other. The performance of the apparatus is very satisfactory. The photographs show the Sun's image sharply defined, but it is only when the sky becomes clear and blue in colour that any coronal appearances present themselves.

In Dr. Huggins's earlier work he employed absorbing media in order to limit the light used to the violet rays. But much difficulty was experienced from the rapid manner in which the coloured solutions (potassic permanganate, or iodine in carbon disulphide) decomposed under the influence of sunlight. Dr. Huggins therefore tried chloride of silver plates, which are strongly sensitive to light between H and h , but hardly at all beyond H, and has been able to secure photographs of the corona with them, without the use of any absorbing medium at all. The developer employed has been a solution of ferrous citro-oxalate, and all the plates have been backed with a solution of asphaltum in benzole. For the purpose of screening the sensitive surface from the intensely bright image of the Sun, small circular disks of thin brass, slightly larger in diameter than the Sun's image, were held close before the sensitive plate. Less advantage was, however, found from the use of the disk than had been anticipated. No photographs could be secured on May 6, the day of the total solar eclipse. One of the observers of the eclipse, however, having made a careful comparison of the short exposure photographs taken at Caroline Island with Dr. Huggins's photographs, expresses his opinion that Dr. Huggins's photographs of the corona are certainly genuine up to 8' from the Sun's limb.

VARIATIONS IN THE LIGHT OF COMET 1883 *b* (PONS-BROOKS).—This comet promises to be one of no ordinary interest, not only on account of its period, but also from its physical character. When first discovered on Sept. 2 it was a very faint and small nebulous object, and was estimated to be of about the 10th magnitude. The following evening the appearance of a stellar nucleus was discovered by Mr. Wendell, who observed it at the Harvard College Observatory. It was observed on Sept. 5 at many places both in Europe and America, and was now certainly not brighter than the 12th magnitude, a faint nearly circular nebulosity, some 40" in diameter. On Sept. 9 M. Bigourdan at Paris detected a small ill-defined nucleus, and the brightness of the comet seemed to that observer to have distinctly increased since Sept. 5 and even to further augment during his watch. On Sept. 21, the comet as seen at Harvard College was, however, still very faint, but the following evening its appearance had so completely changed that "it was difficult to believe it the same object previously seen." It now showed very little nebulosity and resembled a star nearly as bright as the 8th magnitude, and appeared to grow brighter as the evening went on. The next night, Sept. 23, it had again changed its appearance, having lost its stellar appearance, and again appearing as a round nebulosity the central portion of which was not distinctly separated from the rest. It was brighter than on Sept. 22, but scarcely so bright as the 8th magnitude. Traces of the development of a tail were perceptible. On Sept. 24 its diameter was observed at Paris to be nearly 2'. On Sept. 26 its diameter was measured at Greenwich as 2' 20". The comet still preserved its circular shape and was considerably condensed in the centre. On Sept. 27 the nebulosity was much enfeebled, and a nucleus of the 10th or 11th magnitude had distinctly separated itself from it. On Oct. 6 the nucleus was only of the 12th magnitude, nevertheless the comet as a whole was more readily seen than at the beginning of September.

The calculated brightness of the comet on Dec. 13 will be equal to that which it displayed when it was reported as visible to the naked eye during the 1812 apparition, and it will be four times as bright in the middle of next January, the brightness diminishing from that period, until on Feb. 21 it is again of the same amount as on Dec. 13.

THE GEODETIC CONGRESS.—At the Geodetic Congress, which was held at Rome from Oct. 15 to Oct. 24, it was resolved to recommend the adoption as a universal initial meridian of that of Greenwich, longitudes to be reckoned from Greenwich in the sole direction of from west to east. It was also resolved to adopt a universal hour, side by side with the local or national hours, which would necessarily continue to be employed in civil life, the point of departure for the universal hour and for cosmopolitan date being the mean noon of Greenwich, coinciding with midnight or the

beginning of the civil day under the meridian situated at 12 hours, or 180° from Greenwich; the universal hours to be counted from zero to 24.

POTSDAM OBSERVATORY *.—The direction of this observatory was entrusted to Prof. H. C. Vogel on the 1st of April, 1882, and no changes in the *personnel* were made during the remainder of that year.

In the early part of the year some hundreds more stars were spectroscopically examined, completing the zone of spectroscopic survey from -1° to $+20^{\circ}$ of declination. The reduction of these observations is finished, and the printing nearly so.

Dr. Vogel intends to resume these zone observations next year, as this year will be principally devoted to the re-determination of the wave-lengths of a great number of Fraunhofer's lines; considerable progress had been made with this in the first half of 1882; but in consequence of the absence of Drs. Müller and Kempf (who had assisted in it) on one of the Transit-of-Venus expeditions, observations of this kind were not continued during the remainder of it.

Prof. Spörer has continued his observations of the solar spots, and in the course of 1882 observed 250 different groups (including in this term the isolated spots). They were distributed through the months as follows:—

January ..	No. 1-21	July	No. 110-134
February ..	No. 16-40	August	No. 132-158
March	No. 37-61	September..	No. 148-176
April	No. 57-82	October....	No. 169-197
May	No. 79-100	November..	No. 192-215
June	No. 98-119	December..	No. 213-235

The heliographical positions of these spots have been calculated by Dr. Wilsing, who also assisted in the observations. Several photographic plates of the Sun have been measured by Dr. Wilsing, and the positions of the spots thus determined used to complete or check those found by the method of projection.

Prof. Spörer and Dr. Wilsing have also determined the places of a large number of prominences observed on the Sun's limb, and observed 150 prominences.

It is well known that Secchi made some investigations on the amount of heat radiated by different points in the Sun's limb, and found differences in this respect between the pole and the equator. Prof. Spörer has made further observations with this view under favourable circumstances, but has not been able to arrive at any decisive result. He has, however, from a general examination of observations of the prominences as distributed into zones (between 1880, April 12, and 1880, September 3), come to the conclusion that the whole number of prominences in the northern and

* Vierteljahrsschrift der Astron. Gesell. 1883, Heft 2.

southern hemispheres of the Sun respectively is nearly equal, and that the maximum of these occurs in higher latitudes than in the case of the spots, a result agreeing with that found by Tacchini from his own observations.

Dr. Müller observed Wells's comet photometrically on 21 days, from April 21st to June 6th, with the Zöllner photometer on the Steinheil refractor at Potsdam. The results of his observations were noticed in the 'Observatory' for 1882, Oct., pp. 307 and 308.

Photometric observations of comet II. 1882, Dr. Müller was only able to obtain on two nights previously to his departure as chief of one of the German Transit-of-Venus expeditions.

A large number of observations of variable stars was made in 1882; and photometric observations were made of Algol, as well as of Wells's comet, and the planets Mars, Saturn, and Neptune.

Another investigation made by Dr. Müller has been regarding the changes of brightness undergone by different parts of the solar spectrum in consequence of change of the Sun's elevation in the heavens. The interesting result was obtained, that when the Sun was only 3° above the horizon, the blue and violet parts of the spectrum were reduced in brightness about twenty times more than the red part (*Astr. Nach.*, No. 2464).

HONGKONG OBSERVATORY.—The 'China Mail,' in referring to this Observatory, says that Dr. Doberck will first be instructed to draw up a report for the Governor of Hongkong on the minor stations now in existence. He will examine past records, and if these are found fairly accurate, will endeavour to furnish certain data as to the climatic conditions prevalent throughout the colony during the different months of the year. When this is done, it will probably be found feasible to make these stations cooperate with the central station at Kowloon, especially in observations connected with typhoons. Another important item will be the determination of the magnetic elements, and the investigation of the magnetic attraction of the various mountains and hills in the colony and its neighbourhood. It is also possible that, under instructions from the Governor, Dr. Doberck will proceed to Manilla, Shanghai, and other places on the coast of China, to inspect the observatories there, and put himself into communication with the directors of those institutions, with the view of having their reports sent regularly to the Hongkong Observatory to receive careful discussion, these with the object of eventually furnishing trustworthy weather forecasts.

SWIFT'S NEW COMET.—No other observations have been secured of the object Mr. Lewis Swift observed on Sept. 11 and 13, and which he believed to be a new comet.

THE minor planet No. 226 has received the name of Weringia.

Approximate Times of Transit of the great Red Spot and Equatorial White Spot across the Central Meridian of Jupiter in November 1883.

Day of Month.	Red Spot on C.M.	White Spot on C.M.	Day of Month.	Red Spot on C.M.	White Spot on C.M.
	h	h		h	h
1.	14.6	13.1	17.	17.8	12.6
2.	20.4	18.6	18.	13.6	18.1
3.	16.3	14.3	19.	9.5	13.8
4.	12.1	19.8	20.	15.3	19.3
5.	17.9	15.4	21.	11.1	15.0
6.	13.7	11.1	22.	16.9	10.6
7.	19.5	16.6	23.	12.7	16.1
8.	15.4	12.3	24.	18.5	11.8
9.	11.2	17.8	25.	14.4	17.3
10.	17.0	13.4	26.	10.2	13.0
11.	12.9	18.9	27.	16.0	18.5
12.	18.6	14.6	28.	11.9	14.2
13.	14.5	20.1	29.	17.6	19.7
14.	10.4	15.8	30.	13.5	15.3
15.	16.1	11.5	Dec. 1.	9.4	11.0
16.	12.0	17.0	2.	15.2	16.5

The red spot is still visible, though very faint. It was last observed by the writer on October 15, when it came to the planet's central meridian at 15^h 37^m. It was by no means a difficult object, then being far more easily seen than on August 23. At times the shape of the spot came out very distinctly, notwithstanding its feebleness, and the indentation or hollow in the great S. belt, contiguous to the spot, continues a very conspicuous feature.

Mr. A. S. Williams, of Brighton, observed the spot to be central on Sept. 14 at 15^h, and on Sept. 16 at 16^h 37^m.

Signor Riccò's announcement, referred to in your last Number, that on Sept. 10 "the red spot had finally disappeared, the region where it had been now appearing very white," is therefore premature. The spot not only continues visible, but ought to be a tolerably easy object in the 10-inch refractor of the Palermo Observatory, and Sig. Riccò will doubtless redetect it on renewed examination.

The equatorial white spot was last seen at Bristol on October 15 at 17^h 56^m, as it crossed the central meridian of Jupiter. It was very bright and seemingly preserves the conspicuous appearance it presented in 1880. The spot is usually followed by a luminous trail running to the E. along the equatorial margin of the great S. belt.

The red and white spots will come to conjunction on about December 8. The white spot will then have completed 25 revolutions of Jupiter, relatively to the red spot, in the interval of 1114 days, elapsed since Nov. 19, 1880.

Bristol, 1883, Oct. 16.

W. F. DENNING.

Ephemeris of Comet Pons-Brooks.*
By MM. SCHULHOF and BOSSERT.

For Berlin Midnight.

1883.	α			δ			Log A.	Brightness.
	h	m	s	°	'	"		
Oct. 29....	16	59	14.32	+53	7	35.9	0.21905	4.12
31....	17	2	58.28	52	45	21.7		
Nov. 2....	17	6	55.35	52	23	19.7	0.20268	4.73
4....	17	11	5.95	52	1	27.4		
6....	17	15	30.63	51	39	41.7	0.18521	5.48
8....	17	20	9.90	51	17	58.5		
10....	17	25	4.39	50	56	13.3	0.16653	6.39
12....	17	30	14.82	50	34	19.7		
14....	17	35	41.99	50	12	10.9	0.14657	7.51
16....	17	41	26.81	49	49	38.4		
18....	17	47	30.29	49	26	32.6	0.12523	8.92
20....	17	53	53.47	49	2	42.5		
22....	18	0	37.48	48	37	55.6	0.10242	10.68
24....	18	7	43.47	48	11	56.3		
26....	18	15	12.64	47	44	27.4	0.07806	12.92
28....	18	23	6.22	47	15	7.8		
30....	18	31	25.47	+46	43	33.3	0.05208	15.79

The brightness of the comet at the time of discovery is taken as unity.

Differences of Right Ascension and Declination between Iapetus and the centre of Saturn. By A. MARTH†.

$\alpha - A.$			$\delta - D.$	$\alpha - A.$			$\delta - D.$
			δ				δ
Nov. 1		-18.0	+ 90	Nov. 16		-40.0	-154
2		-20.9	+ 74	17		-39.4	-166
3		-23.7	+ 57	18		-38.5	-177
4		-26.3	+ 40	19		-37.3	-186
5		-28.7	+ 23	20		-35.8	-194
6		-31.0	+ 6	21		-34.1	-201
7		-33.1	- 12	22		-32.1	-206
8		-34.9	- 30	23		-29.9	-210
9		-36.5	- 47	24		-27.5	-212
10		-37.8	- 64	25		-24.9	-213
11		-38.9	- 81	26		-22.2	-213
12		-39.7	- 97	27		-19.2	-211
13		-40.2	-113	28		-16.2	-207
14		-40.4	-127	29		-13.0	-202
15		-40.3	-141	30		- 9.8	-196

* Astron. Nachr. No. 2546.

† Monthly Notices, Vol. xliii. No. 8.

*Greenwich Mean Times of the East Elongations of the Seven
Inner Satellites of Saturn.*

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS(East).	ENCELADUS.	TETHYS.	DIONE.
h	h	h	h
Nov. 5 17.7	Nov. 2 1.8	Nov. 2 17.7	Nov. 1 6.9
6 16.3	3 10.7	4 15.0	4 0.6
7 14.9	4 19.6	6 12.3	6 18.2
8 13.5	6 4.5	8 9.6	9 11.9
9 12.1	7 13.3	10 6.9	12 5.5
10 10.7	8 22.2	12 4.2	14 23.2
11 9.3	10 7.1	14 1.4	17 16.8
22 16.8	11 16.0	15 22.7	20 10.5
23 15.4	13 0.8	17 20.0	23 4.1
24 14.0	14 9.7	19 17.3	25 21.8
25 12.6	15 18.6	21 14.6	28 15.4
26 11.2	17 3.5	23 11.9	
27 9.8	18 12.3	25 9.2	RHEA.
28 8.4	19 21.2	27 6.5	Nov. 1 9.5
29 7.1	21 6.1	29 3.8	5 21.9
	22 15.0		10 10.2
MIMAS(West).	23 23.8	TITAN.	14 22.5
Nov. 17 12.3	25 8.7	Oct. 31 2.7	19 10.9
18 10.9	26 17.6	Nov. 16 0.1	23 23.2
19 9.5	28 2.5		28 11.5
20 8.1	29 11.3	HYPERION.	
21 6.7	30 20.1	Oct. 21 4.0	
30 17.0		Nov. 11 9.1	

The following table will furnish the means for inferring approximate distances and position-angles of the satellites from the diagram given in Plate I.

	Position-angle of Minor Axis of Ring.		Semi-major axis of orbit of Titan.
Oct. 31	356° 0'	Oct. 31	205".5
Nov. 10	356 5	Nov. 10	207.3
20	356 11	20	208.4
30	356 18	30	208.7

Astronomical Memoranda, 1883, November.

Sun. Nov. 1, sets 4^h 32^m, rises 18^h 57^m; Nov. 11, sets 4^h 15^m, rises 19^h 15^m; Nov. 21, sets 4^h 2^m, rises 19^h 30^m; Nov. 30, sets 3^h 53^m, rises 19^h 46^m.

Equation of Time:—Sun *before* Clock, Nov. 1, 18^m 16^s; Nov. 11, 15^m 52^s; Nov. 21, 14^m 2^s; Nov. 30, 11^m 14^s.

Sidereal Time at Mean Noon:—Nov. 1, 14^h 41^m·7; Nov. 11, 15^h 21^m·2; Nov. 21, 16^h 0^m·6; Nov. 30, 16^h 36^m·1.

Moon.			sets.			rises.		
	h	m		h	m		h	m
Nov. 1..	5	26	Nov. 11..	15	56	Nov. 21..	11	48
2..	6	6	12..	17	18	22..	12	54
3..	6	50	13..	18	40	23..	13	59
4..	7	42	14..	<i>rises.</i>		24..	15	2
5..	8	41	15..	5	10	25..	16	5
6..	9	44	16..	6	8	26..	17	8
7..	10	53	17..	7	13	27..	18	9
8..	12	4	18..	8	21	28..	19	7
9..	13	20	19..	9	32	29..	<i>sets.</i>	
10..	14	37	20..	10	41	30..	4	47

First Quarter, Nov. 7, 12^h 4^m; Full Moon, Nov. 14, 4^h 37^m; Last Quarter, Nov. 21, 1^h 44^m; New Moon, Nov. 29, 6^h 54^m.

Mercury is a morning star in the early part of the month. He is in superior conjunction with the Sun, Nov. 25, 18^h, and in aphelion Nov. 30, 1^h.

Venus is an evening star, in her descending node, Nov. 8, 3^h. Diameter:—Nov. 1, 10^{''}·0; Nov. 30, 10^{''}·4. Illuminated portion of disk, 0·972 on Nov. 15.

Nov. 1, R.A. 15^h 8^m·5, Dec. 17° 20' S., tr. 0^h 27^m, sets 4^h 59^m
30, 17 41 '4, 24 16 S., 1 5 4 53

Mars passes from Cancer into Leo about the middle of the month. Diameter:—Nov. 1, 9^{''}·1; Nov. 30, 11^{''}·3. Illuminated portion of disk 0·895 on Nov. 15.

Nov. 1, R.A. 8^h 45^m·9, Dec. 19° 38' N., tr. 18^h 3^m, rises 10^h 13^m
30, 9 28 '6, 17 30 N., 16 50 9 13

Jupiter is stationary, Nov. 21, 3^h. Diameter:—Nov. 1, 36^{''}·6; Nov. 30, 39^{''}·7.

Nov. 1, R.A. 8^h 25^m·3, Dec. 19° 36' N., tr. 17^h 41^m, rises 9^h 51^m
30, 8 27 '4, 19 34 N., 15 49 7 59

Saturn is in conjunction with Aldebaran Nov. 1, 17^h, and in opposition to the Sun, Nov. 28, 16^h.

Nov. 1, R.A. 4^h 29^m·6, Dec. 19° 47' N., tr. 13^h 49^m, rises 5^h 54^m
30, 4 20 '2, 19 26 N., 11 42 sets 19 31

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	
Nov. 15	46 ^{''} ·22	20 ^{''} ·02	30 ^{''} ·73	13 ^{''} ·31	18 ^{''} ·4
Dec. 5	46 '35	19 '97	30 '82	13 '28	18 '4

Neptune is in opposition to the Sun, Nov. 11, 21^h.

Nov. 1, R.A. 3^h 12^m.2, Dec. 16° 0' N., tr. 12^h 28^m, rises 5^h 0^m
30, 3 8.9, 15 47 N., 10 31, sets 17 58

Phenomena.

		G. M. T.	
		h	m
Nov. 1	11	52	J. ii. Tr. E.
	12	6	J. iii. Ec. R.
	13	52	J. iii. Oc. D.
	17	26	J. iii. Oc. R.
5	16	50	J. i. Tr. I.
6	12	49	J. i. Ec. D.
	14	26	J. ii. Ec. D.
	16	22	J. i. Oc. R.
7	11	18	J. i. Tr. E.
	13	38	J. i. Tr. E.
8	10	50	J. i. Oc. R.
	11	28	J. ii. Tr. I.
	12	46	J. iii. Ec. D.
	14	22	J. ii. Tr. E.
	15	28	J. iv. Oc. D.
	16	4	J. iii. Ec. R.
	17	44	J. iii. Oc. D.
11	13	58 60	Piscium
			Oc. D. 159°.
12	11	14	J. iii. Tr. E.
15	51	54	Ceti
			Oc. D. 70°.
13	14	42	J. i. Ec. D.
	17	2	J. ii. Ec. D.
	18	13	J. i. Oc. R.
14	8	12	B. A. C. 1119
			Oc. D. 67°.
	9	2	B. A. C. 1119
			Oc. R. 322°.
	13	9	J. i. Tr. I.
	14	52	B. A. C. 1206
			Oc. D. 98°.

		G. M. T.	
		h	m
Nov. 14	15	29	J. i. Tr. E.
	15	57	B. A. C. 1468
			Oc. R. 273°.
15	12	40	J. i. Oc. R.
	13	55	J. ii. Tr. I.
	16	44	J. iii. Ec. D.
	16	50	J. ii. Tr. E.
16	9	57	J. i. Tr. E.
17	11	39	J. ii. Oc. R.
19	11	24	J. iii. Tr. I.
	12	19	A ² Cancri
			Oc. R. 230°.
	14	59	J. iii. Tr. E.
20	16	35	J. i. Ec. D.
21	14	59	J. i. Tr. I.
	17	19	J. i. Tr. E.
22	11	4	J. i. Ec. D.
	14	30	J. i. Oc. R.
	16	21	J. ii. Tr. I.
23	9	27	J. i. Tr. I.
	11	47	J. i. Tr. E.
24	14	6	J. i. Oc. R.
25	12	38	J. iv. Oc. R.
26	15	5	J. iii. Tr. I.
	18	40	J. iii. Tr. E.
27	18	29	J. i. Ec. D.
28	16	48	J. i. Tr. I.
29	12	57	J. i. Ec. D.
	16	18	J. i. Oc. R.
	18	44	J. ii. Tr. I.
30	11	16	J. i. Tr. I.
	13	35	J. i. Tr. E.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

Publications received:—A. Gaillot, *Sur la mesure du temps. Réponse aux observations de M. E. J. Stone* (Comptes Rendus, Vol. xevii. No. 9)—*Jamaica Weather Reports for the Months of June, July, August, and September 1883*, also *Jamaica Rainfall from about 1870 to the end of 1879*. Part I. (Government Printing Establishment, Kingston, Jamaica)—*The Science Monthly* (David Bogue and E. W. Allen, London)—*Science Observer, Special Circulars* Nos. 38–41.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 80.

DECEMBER 1.

1883.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1883, November 9.

E. J. STONE, M.A., F.R.S., *President*, in the Chair.

Secretaries: J. W. L. GLAISHER, M.A., F.R.S., and
E. B. KNOBEL.

THE Minutes of the last Meeting were confirmed.

Mr. Knobel announced that 155 presents had been received since the last meeting. Among these was Sir John Herschel's 'Outlines of Astronomy,' translated into Chinese by Mr. Wylie, well known for his researches in Chinese astronomy, and presented by Mr. Thomas Jenner.

The thanks of the Meeting were unanimously voted to the respective donors.

Mr. Knobel read a paper by the *Rev. S. J. Johnson* on "Jupiter without visible Satellites." It gave an account of an observation made in order to ascertain the accuracy of a statement in the 'Nautical Almanac,' that on October 14 the satellites would all disappear. With an equatoreal of $3\frac{1}{4}$ inches aperture and power 180 the planet appeared devoid of its satellites for about half a minute; but probably the satellites might have been seen with a larger telescope.

Mr. Knobel read another paper on the same subject by *Dr. Wentworth Erck*, who stated that the simultaneous disappearance of the satellites did not take place, there being an error of 24 minutes in the 'Nautical Almanac' time of egress of the fourth satellite. The fourth satellite passed off the disk just as the third satellite entered.

Mr. Knobel said that the two papers were consistent with each other, as *Mr. Johnson* thought it possible that with a larger instru-

ment (Dr. Erck's telescope being a $7\frac{1}{2}$ -inch equatoreal with powers 100 and 250) it might have been seen that the satellites were never both entirely on the disk at the same moment.

Mr. Knobel also read a paper from *Major-General Tennant* on "Humidity as a cause of Variation in the rates of Chronometers."

The paper stated that *Major-General Tennant* had borrowed from the Government a chronometer, No. 2684, by *Thomas Fletcher*, in March 1882. At first it had a gaining rate of 6.5 seconds a day; and this rate continued till the end of May, when for 20 days there was a constant fall to -2 seconds. The rate then rose nearly one second for a week, and it had continued to vary in a similar manner up to a recent date. He had compared these rates with the published mean temperatures at the *Calcutta Observatory*, but on examination it was seen that the variations in the rate depended on the temperature only to a small extent. The chronometer had stood in a room sheltered from the sun, but exposed to the air. He had finally come to the conclusion that some of the periodic changes were due to humidity. His first suspicion had been aroused by a sudden fall of rate coincident with the first heavy rain this year. The same phenomenon had occurred again at the end of the rains, when the soil was loaded with moisture. He thought that if the oil used in the chronometer were at all hygroscopic, it was easy to see that it might become more fluid in damp weather, and that the temporary humidity of the air might thus affect the rate. But proof of a connexion could only be obtained by special experiments and comparisons.

Mr. Lecky. I understand the chronometer was not clean when *General Tennant* received it.

Mr. Knobel. It had been some time in India, and had not been cleaned since its arrival there.

Mr. Lecky. I think it is more likely that the humidity affected the dirt in the chronometer than the oil.

Mr. De La Rue. Water is not so antagonistic to oil as is often supposed. For example, if you have a steady surface of water, and let fall a small drop of oil upon it, it immediately spreads out, showing that there is a strong attraction between oil and water. Again, if you put two pieces of wood close together, and let fall a drop of oil between them, the two pieces of wood are parted with considerable force. It is therefore not at all impossible that moisture might adhere to the surface of the oil and have some effect on the rate of the chronometer.

The President asked *Major Herschel* if he had any remarks to make on the paper.

Major Herschel replied that he had had a chronometer under observation for the last twelve months, and had been very much surprised to observe what sudden variations of rate it showed, amounting to one or two seconds a day, without any apparent reason. He was decidedly of opinion that the rate of a chrono-

meter from moment to moment required more examination than it had received up to the present time.

Mr. Knobel. Did you compare the meteorological conditions ?

Major Herschel. No ; the chronometer was kept in a room.

Mr. De La Rue. The experiments might be very well repeated, and the effect of temperature be eliminated from that of moisture by placing the chronometer in a case dried artificially by chemical means. The rates at different temperatures should then be ascertained first of all, and afterwards moisture should be introduced into the case. The paper is certainly a very suggestive one, and every thing that General Tennant does is done so carefully that we ought not to discard any of his observations from our careful consideration.

Mr. Dunkin. I have a short paper on a subject which is rather of a technical character ; but though it may not be interesting to Members generally, it is of very great importance. I have drawn it up in consequence of noticing some remarkable changes in a table printed in the paper of Mr. Stone in the May number of the 'Monthly Notices,' in which he has given the mean excess of tabular errors of longitude founded on the Greenwich meridian observations of the Sun from 1864 to 1880.

The Paper (which was partly read) stated that it was highly important that any explanation of the apparent increase of the tabular errors in the right ascension of the Sun should be placed on record. Mr. Stone considered that the general increase was clearly progressive, and amounted to $0''\cdot052$ per annum, and he believed that it was the effect of the change of unit adopted by Le Verrier in his solar tables. This apparent positive increase in the observed tabular errors might be accounted for as the effect of personality in the observers. It so happened that between 1872 and 1875 several important changes took place in the regular observing staff of the Greenwich Observatory. The observers who had been in the habit of giving systematically large negative errors of the Sun had been succeeded by others who, on the contrary, gave large positive errors. The existence of a peculiar personality in observing the limbs of the Sun and Moon had been well known for many years, and had formed the subject of a former communication to the Society. In the present paper there was a table which exhibited very clearly the errors of the R.A. of the Sun, determined at Greenwich by each observer from 1864 to 1881. These errors had been collected together and then arranged separately in different groups, according to the observers. Mr. Dunkin's observations, for instance, from 1864 to 1869, were in one group. There were also, in separate groups, the observations of Mr. Ellis, Mr. Criswick, and Mr. Carpenter. Mr. Stone had given in his paper the mean of the tabular errors in longitude for each year from 1864 to 1880, and it was a remarkable fact that after 1872 the errors appeared constantly to increase, and became at length three, four, five, and six times larger than those in pre-

ceding years. There must be some cause for this. Mr. Stone believed that the increase was caused by the change of unit which Le Verrier has adopted in the Solar tables, but it appeared that it might be explained by personalities of the observers.

After giving an explanation of the general table of tabular errors, and pointing out those years where changes in the observers had occurred, Mr. Dunkin remarked that he had taken the mean of all the tabular errors of the R.A. of the Sun for each year from 1864 to 1881. These mean errors were then corrected for the effect of personality due to the changes of observers. From the comparison of the yearly corrected and uncorrected errors very little evidence is shown of any change in the tabular error of the R.A. of the Sun from 1864 down to 1881. It might be said the corrected errors showed a residual which would indicate a change; but that was really not so, because, though the errors appeared larger at the end than at the beginning, this might be explained by remarking that the errors determined from the observations of the computers are largely affected by personality in a positive direction, and therefore they increased the mean positive errors, particularly in later years. No sensible change had been found during many years in the errors determined from Mr. Criswick's observations; but they ought to have shown a positive increase in the error of the right ascension of the Sun if such an error had in reality been in progress. Mr. Criswick's observations give the following result:—

	Arc.
1864 to 1869, mean tabular error	+0".36
1870 to 1875, " "	+0".32
1876 to 1881, " "	+0".31

while the right ascension computed from Le Verrier's tables had a mean error for the same years amounting respectively to 0".0, +0".4, and +0".7, the apparent progressive increase being due to personality in the observers as explained in the paper.

The President. Mr. Dunkin's paper is an interesting one so far as it relates to the existence of personal equations between different observations; but I think he has overrated the importance of the evidence it supplies with regard to the change which I believe has been made in the unit of time. The whole effect of the error is only to produce a change of 0".06 per annum in the tabular R.A. of the Sun, a quantity easily obscured by questions of personality and errors of observation. The evidence that Mr. Dunkin has brought forward is strong up to a certain point; but the whole quantity is so exceedingly small that, in face of the large discordances between observers, I think it would be difficult to answer for 0".8. Small though it is, however, I am not by any means satisfied that it is not shown by the observations. If we take the mean errors of all the observers, it seems distinctly shown; and though Mr. Dunkin shows that, when certain corrections for personal differences in the mode of observing the Sun are applied, the

change in the error of the Sun's tabular longitude is by no means so well marked, amounting only to about $0''.4$ instead of $0''.8$, and that in the case of Mr. Criswick's observations, taken alone, no difference is perceptible, I am by no means certain that we can be sure that all these observers have always observed in the same direction. In the case of the Moon, however, the error produced is about thirteen times as great, and is therefore a quantity entirely removed from all questions of error of observation.

The Astronomer Royal. I think Mr. Dunkin's paper is of greater importance than Mr. Stone implies, for it shows that we cannot discuss mean results of observations of the Moon and Sun without taking into account the personalities of the observers concerned. The quantities that Mr. Dunkin has brought in evidence (in continuation of his former papers on personality in observations of limbs of the Moon and the Sun) are quantities really not at all small considered in the light of modern astronomy. The difference between different observers amounts to 2 seconds of arc, which is not an insignificant quantity. Mr. Dunkin shows that there may be a very great apparent jump from one year to the next produced by a change of observers; and if we are to reduce the apparent errors of the tables to one homogeneous system we must take account of the personalities of the observers concerned. With regard to the computers, the question is one of relative personality—not whether they give too large an absolute error, or too small, but what they give relatively to the regular observers. Mr. Dunkin has shown that the apparent increase in the mean error of the Sun is to be accounted for by a simple change of one or two observers; and similar effects will be produced in the case of the Moon. Therefore, in discussing the mean errors of either the Sun or the Moon, the personality of the observers ought to be taken into account.

Mr. Downing. In order to determine the errors of the tables accurately, it would be desirable to discuss observations made at different observatories. It is hardly safe to say an error is of such-and-such a magnitude if you look at the results obtained at one observatory only. There is no doubt a systematic error attached to each instrument as well as to each observer; and these errors should be eliminated before any attempt is made to correct the tables. This error may not be constant, because it may change on account of wear of the instrument; in fact, we have had an instance of that at Greenwich in the effects of wear on the transit circle microscope-micrometers, so that it would be desirable to try to eliminate the instrumental error as well as the personal error in order to determine the errors of the tables.

The Astronomer Royal. I quite agree with Mr. Downing in his remarks; and I may perhaps quote a remark of Le Verrier that, in the present state of astronomy, it is important to multiply observers and observatories rather than observations for the discussion of the errors of the Moon and Sun.

The President. I have always regarded it as one of the strong

points of the Greenwich observations that fundamentals are referred to the mean habits of many observers rather than to a single observer; there may, in this way, be produced some appearances of roughness in the individual results; but I believe that there is less liability to change of mean habit than in the habit of any selected observer over a long series of years. I think it quite possible that as a person gets older his habit of observing may change. We do know of such cases having occurred. I prefer therefore to trust to mean results rather than to assume that a habit of observing has remained unchanged during 17 years. At all events, it will be seen that there are two sides to the question. The evidence which can, under any circumstances, be afforded by these Sun observations is slight as compared with that of the Moon; and it was only after my theoretical result had been confirmed by the lunar observations that as a matter of curiosity I tried whether these solar observations could indicate so small an effect as that under discussion, and they certainly appeared to do so.

The Astronomer Royal. The errors of the Sun do not show a progressive increase.

The President. They show, on the whole, an increase. You cannot expect, in a few years, to determine an error of $0''.05$.

The Astronomer Royal. The increase should amount to $1''$ in 16 years.

The President. The progress for the year is only $0''.052$. It is so small a quantity as to be scarcely perceptible in a few years; so that I am by no means so thoroughly impressed with the agreement of Mr. Criswick's results. It is very striking and curious, but I am by no means certain that no change has taken place in the case of the Sun's longitude.

Mr. Dunkin. With regard to the table in Mr. Stone's paper, which is certainly an important one, the mean is taken of all the errors of longitude as given in the Greenwich observations for each year. The errors are really almost constant within $\pm 0''.1$ or $0''.2$ up to 1872, when in 1873 they jump from $0''$ to $+0''.4$, and from 1873 to 1874 again to $+0''.7$. Now what I wish to point out is that between 1870 and 1875 there was a change from observers who gave negative errors to observers who gave positive errors. In 1873 there was a jump from $0''$ to $+0''.4$, when Mr. Carpenter had retired and Mr. Downing had succeeded him with a very large positive error, and therefore the change from $0''$ to $+0''.4$ is clearly owing to the personality of the observers, and also that succeeding increases in the mean error occurred at subsequent changes in the observers. With regard to the constancy of personal equations, it is true in some respects that as we get older we are more likely to change; but on looking back at my own observations from 1846 to 1870, which is a long period in one's life, I find my own personal equation had scarcely changed at all. I think also that little or no sensible difference has occurred in Mr. Criswick's personal equation in observing transits from 1855 to 1881, so that it is

hardly correct to cast any reflections on Mr. Criswick's observations, because they appear to be very good.

General Walker related that, having been engaged with a staff in making observations to determine longitudes for the purposes of the Electric Telegraph, he knew that personal equations varied a good deal; and one of the staff excused himself on the ground that he had been on a sea voyage and had suffered from sea-sickness, and that they could not expect his personal equation to remain constant under such circumstances. (Laughter.)

The President. I have a paper here containing the numerical evidence with regard to this question of change of the unit of time, which is afforded by the Greenwich Lunar observation from 1864 to 1881. I have supposed that the term which is indicated by my theory is a real term; and I allow for the motion of Moon, both in R.A. and N.P.D., in the time thus indicated by theory. In that way I obtain what I call the corrections to be applied to the tabular places before they are compared with the observations. The error in time, which is small in 1864, increases to a very large quantity in 1881, amounting to about 27 seconds of time. I have computed the corrections in a straightforward manner, and the whole of the results are given in this table. Whether my theory is right or wrong, my facts are certainly indisputable. I have already given a table which shows how small were the corrections required by Hansen's tables to represent the observations from 1847 to 1863, before any change was made in the adopted unit of time. Of course, there may be a difference of opinion in regard to these facts proving my point; but my object is to bring forward this evidence. I have, in this table exhibited on the board, given the N.P.D. observations for 1880, as freer probably from personality than the R.A. observations, and because the N.P.D. suffers rapid changes, the tabular quantity sometimes rapidly increasing and sometimes decreasing within a year. Then if the times at present in use are relatively too great we should expect the tabular N.P.D. to be too large when the N.P.D. is increasing and too small when the N.P.D. is decreasing, and in 1880 by amounts which are far too large to be lost in mere errors of observation or personality. Now such is found to be the case. The tabular quantities are larger than the observed N.P.D. whenever the tabular quantities are increasing, and too small whenever the tabular quantities are decreasing; and when the corrections indicated by my theory are applied, the errors of Hansen's tables both in R.A. and N.P.D. are very nearly as small as those which are indicated by the observations of 1847-1863. It must be remembered that I have introduced no constant to obtain this agreement. That a correction,

$$\frac{360 \cdot 60 \cdot 60}{15} \times \frac{\delta n}{n} \times t, \text{ applied to all the Greenwich times (where}$$

δn , n , and t are all quantities over which I have no control, n being the Sun's mean motion in longitude, δn the exact difference between Le Verrier and Bessel's adopted values of n , and t the time

after which the change from n to $n + \delta n$ was made, viz. 1861, Jan. 1), should produce corrections which destroy the large apparent discordances between Hansen's tables and observations may, of course, be a matter of accident; but, if so, it is certainly the most remarkable accident I have ever met with in my experience; and that such a correction should result from a theory which is supported by mathematical proof, the errors of which (if there be any errors, which I dispute) those who refuse to accept my results have hitherto failed to point out, is still more remarkable as the result of accident. All my theory, in fact, assumes is that when we build up a theory of astronomy on the assumption that the mean longitude of the Sun increases n in every unit of time, and afterwards from any cause whatever assumes that the mean longitude of the Sun shall increase $n + \delta n$ in every unit of time, then, since $10000 n$ is certainly not equal to $10000 n + 10000 \delta n$, the unit of time in the second case must be a longer time than in the first case in the ratio of $1 + \frac{\delta n}{n} : 1$. If this be assumed, all my

results follow; and that such a theory should account for the remarkable increase of the errors shown by Hansen's tables since 1864 is at least a strong reason why my views should receive most careful consideration. I need hardly point out that calculations based on the assumption that no change in the adopted unit of time has taken place cannot prove that my results are wrong.

It may, of course, be said that, although the agreement between your results and observations is very remarkable, that it is quite possible that the present errors of Hansen's tables are due to the neglect of or the wrong introduction of large inequalities. Of course such a supposition is quite possible, however improbable; but if you recall the facts of the case you will find that this explanation can hardly be the true one. When Hansen made his tables and discussed the observations from 1750 to 1858, he certainly did represent the whole of the observations during that period with very considerable accuracy indeed; and there is, so far as I know, only one point that seriously affects this question about empirical inequalities. He introduced two long inequalities—one with a coefficient of $15''$, and another of $27''$. With respect to one of these inequalities, it was afterwards reinvestigated by Delaunay, and Hansen's results were confirmed, and we may fairly regard it as part of the theory. With respect to the other inequality, the case is different. Hansen had found two values by two methods, one large and the other small. Delaunay reinvestigated it, and found a small value, and Hansen does not appear to have reinvestigated the matter. But the point I wish you to bear in mind is this: that, whether that inequality be real or not, it was an inequality that was found absolutely necessary to make Hansen's theory to represent this long series of observations. But within thirteen years of the adoption of Hansen's tables, the errors have got so large that his tables ap-

peared nearly as bad as Burckhardt's, which had to be abandoned. Professor Newcomb some time since took up this subject in his researches on the motion of the Moon, and attempted, first, to modify the constants involved in Hansen's tables, so as to represent all the observations. This he found impossible, nor could he represent the observations from 1750 to 1876, only a few years more than Hansen included, in this discussion. He found it impossible, in fact, to represent these observations either by an alteration of the epoch or by an alteration of the proper motion. What he has done, then, is to reject the doubtful long inequality of Hansen, but that did not do either; and then he had to introduce another empirical inequality, and for this purpose he adopted the one with the same period as that investigated by Hansen and Delaunay. But this empirical inequality had an epoch utterly at variance with that adopted by Hansen and found necessary to represent the observations up to 1863. By the introduction of a new empirical inequality, a new mean motion, and a new epoch you can represent the modern observations very well for a time. I have not mentioned this to find fault with what Professor Newcomb has done, but to bring forward prominently this: that if it is absolutely necessary, after the extension of a few years in lunar observations, not only to change the epoch and the mean motion, but to introduce a new empirical inequality, and if these empirical inequalities have no relationship one to another, with no apparent connection or slight modification of the epoch and coefficients, and do not therefore merge gradually one into another, then these long inequalities have no real existence in the theory, and some most serious source of error, quite independent of such long inequalities, has affected our work. I have said this to show you the difference between the results I have given here and those results that hang upon three or four disposable constants. This theory of mine has nothing of mine in it. It results simply and straightforwardly from one very simple assumption, that the Sun's mean motion is continuous; and if I am right in that, then this term I bring forward is, and must be, applied. I feel confident in my theory; but, independently of the theory altogether, I wish to call attention to the fact, which can, if you please, be considered quite independently of theory.

The Astronomer Royal. I do not wish to enter into any discussion of Mr. Stone's theory, because that subject has been pretty well thrashed out, and it is quite unsuitable for discussion at these meetings. But I may point out, in regard to the errors in N.P.D., that, if I understand the question rightly, they simply mean that there is a certain error of the Moon in longitude which will produce a corresponding error in N.P.D. as well as in R.A., so that when the N.P.D. is increasing, if the Moon has a *plus* error in longitude, it will produce a *plus* error in N.P.D., and *vice versa*. The discussion of the errors of N.P.D. which Mr. Stone has made simply shows that they arise from the well-known fact that the

Moon has a large error in longitude at present; but, beyond that, I do not see that it supports any one theory more than any other which professes to account for the origin of that error in the Moon's longitude. That error may arise from various causes.

The President. Will you please to indicate the causes?

The Astronomer Royal. That I cannot undertake to do specifically, for it has puzzled many people. I should say generally that the cause is the imperfection of Hansen's lunar theory. Attention has been directed very much of late to the mean error in the longitude of the Moon; but there are also large fluctuating errors of the Moon, which may vary by 10 seconds of arc in the course of the year.

The President. The whole of the single observations in R.A. and N.P.D. are given here—the corrected quantities.

The Astronomer Royal. I am not criticizing Mr. Stone's results in detail; but I am calling attention to the fact that there are admittedly imperfections in the lunar tables.

The President. I do not suppose we shall get a perfect lunar theory in our time; but these errors are now reduced to so small a quantity that there seems no justification for the introduction of unknown causes of action at all. Of course, as Mr. Christie, says, it is true that an error in longitude will produce an error in N.P.D.; but, in fact, you could not have an agreement of the kind shown unless you had the variable part of the longitude agreeing with the observed. These are not the mean results. It shows that the actual single longitudes are accurate, and the necessity of the existence of the error in longitude is traced to a distinct cause, which is shown to be sufficient to account for the fact.

The President called attention to the fact that the rooms had been newly decorated, and added that the cost had been within the sum allowed by the Council for the purpose.

The following papers were also announced and partly read:—

E. B. Powell. "Measures of Southern Double Stars."

Prof. A. Hall. "Note on the Mass of Saturn."

Prof. S. Newcomb. "Note on some Observations of the Transits of Mercury and Venus made in Australia by Mr. Russell and Mr. Tebbutt."

J. Tebbutt. "Observations of the Variable Star R Carinæ from May 1880 to Sept. 1883."

F. J. Parsons. "Elliptic Elements of Comet 1882 I. (Wells)."

W. F. Denning. "Showers of large Meteors."

The Astronomer Royal. "Observations of Comet 1883 b (Pons-Brooks) made at the Royal Observatory, Greenwich."

The Ballot was then proceeded with, and the following Astronomers were duly elected Foreign Associates of the Society:—

Prof. S. P. Langley, Allegheny Observatory, Pennsylvania; Prof. J. A. C. Oudemans, The Observatory, Utrecht; Prof. P. Tacchini, Collegio Romano, Rome; Prof. E. Weiss, Imperial Observatory, Vienna.

The following gentlemen were duly elected Fellows :—James T. Cooke, M. Alexandre de Gothard, M. Eugène de Gothard, Richard Ladd, and Robert Rawson.

The Meeting adjourned at 9^h 40^m.

Meeting of the Liverpool Astronomical Society.

THE second Meeting of the present Session was held on Monday, Nov. 12, Isaac Roberts, Esq., F.R.A.S., in the Chair. Seventeen Candidates were proposed and elected Members of the Society. The Chairman read a paper on Jupiter without Satellites. After illustrating, by means of an ingeniously-contrived model, the movements of the four satellites round Jupiter, he described his observations on 14 October of this year at Maghull. The evening was extremely unpromising, as it rained until 3^h 40^m, when it suddenly cleared up. At 5^h 15^m, sidereal time, the third satellite was in transit; the fourth was also in transit, but was going off the disk. The two satellites appeared at this time as protuberances on each side of the planet. Two or three minutes later the three satellites had disappeared entirely, and the fourth had moved off the disk. Before its reappearance he observed a white spot of light close to the W. edge of the planet: this was the second satellite. The following were the errors of the times, as compared with the times given in the 'Nautical Almanac':—

	m	s	
Sat. IV. at egress	23	9	before.
„ III. at ingress	3	6	„
„ I. at reappearance	1	25	„
„ II. at egress	9	41	„

It was therefore evident that there was room for improvement in the Ephemeris of the satellites of Jupiter. Some discussion took place as to the differences between the times of observation and the apparent times as given in the 'Nautical Almanac,' and it was pointed out that the errors were all in one direction.

Mr. Joseph Baxendell, F.R.A.S., fully confirmed the Rev. J. Jevon Perry's statement that excellent definition occurred during an east wind. He was working some years ago with a reflector of 7-inch aperture, and, within a mile of the Manchester Exchange, found the definition excellent with a N.E. wind. The Rev. T. E. Espin remarked that his experience at Wallasey was quite the reverse, it being generally impossible to obtain any definition with

an E. wind. The matter was of great interest, and showed how much depended on the place where the observations were made.

Mr. W. H. Gage, F.R.A.S., read a paper on "July meteors," contributed by Mr. Denning. Captain Tupman first noticed a large number of meteors in 1870 at the end of July, and found a radiant-point near δ Aquarii. Schmidt also made similar observations; Mr. Denning had found the radiant-point R.A. 341° , 14° south in 1878 from observations of fifty-four meteors. The motion was slow, and the path was long, because they were seen near the horizon. In 1879 he observed forty-five meteors in two hours, and fourteen belonged to this radiant. In 1883, in $3\frac{1}{2}$ hours, 48 were observed, and 19 belonged to this stream. The observations should be followed up, as observers differed greatly on the radiant-point. Sawyer found it at 330° and 6° south, while Cruls, in the same year, placed it only five degrees from Fomalhaut. The time of the maximum was also not settled. Mr. Gage made a few remarks on some meteors he had observed at the Society's observatory. He suggested that more care should be taken in obtaining places, since, if the places were accurately given and the stations sufficiently far apart, it would be possible to calculate with some certainty the height of the meteor.

The Rev. T. E. Espin read a paper on further attempts at obtaining the actinic light of the stars, by means of photography, at the Society's observatory. Two plates had been secured, one of Cassiopeia and one of Taurus, showing the Pleiades and Saturn. As a rule, the impression on the plate corresponded very fairly to the magnitude; but there were some very remarkable exceptions. The stars would fall into three classes:—(1) stars whose chemical light was in excess of the light as apparent to the naked eye; (2) stars where it was equal; (3) stars where it was inferior, allowing a difference of 0.2 above or below the apparent eye-magnitude to be due to errors of observation. From the Taurus plate, out of 41 stars he had compared, there were 15 stars whose actinic magnitudes could not be made to correspond with the eye-magnitudes. The difference in some cases was very considerable, and amounted to a whole magnitude. Saturn was photographed on the same plate, and, after a very careful comparison, he found the actinic brightness about equal to that of γ Tauri. Diagrams were shown of Cassiopeia, according to Heis's 'Atlas Cœlestis,' and of the same constellation according to the photograph. Among several interesting differences, α Cassiopeia was reduced to the size of η Cassiopeia, while the magnitude of κ was increased.

Mr. Davies (the Secretary) congratulated Mr. Espin on his success. He said members would hardly realize at what a cost of labour these simple negatives had been obtained. Outdoor observation in damp or frosty weather was not inviting; but each of these plates required an exposure of one hour and a half, and Mr. Espin had been lying on the ground following a star with the finder all the time. Mr. Roberts remarked that, on examining the

Cassiopeia plate with a magnifier, the stars appeared perfectly round, showing how truly they must have been followed in the finder.

The Meeting adjourned soon after 10^h.

The Photographic Photometry of Stars.

EVERY ONE will admit that astronomy not only may derive in the future many benefits from the application of photography, but that even now this art is of great service in many branches of the science. But, granting all this, it may well be open to serious doubt whether photography will be of essential help in the determination of the photometric relations of celestial bodies, and particularly of the fixed stars. In my opinion, at least, a great many special researches on different subjects, of which our knowledge is still in the highest degree imperfect, must precede our being able to apply the photographic method with certainty and success in this direction.

So far as I know, the application of photography to the determination of the relative brightness of stars was suggested, for the first time, in 1857 by Prof. G. P. Bond, at Cambridge (U. S. America). It is well known that this able astronomer deduced from photographic negatives of the stars ζ and g Ursæ Major (Mizar and Alcor) their relative positions with so high a degree of accuracy that, in this respect, the mean values were hardly inferior to the differential determinations made by W. Struve. Perhaps this success contributed to lead the late Prof. Bond to express the hope that, at no distant time, astronomers would succeed in the determination of the relative brightness of the stars, by measuring the diameters of their images on the photographic plates, adopting as a principle that those diameters bore a certain relation to their brightness. Probably Bond did not entirely overlook all the difficulties connected with the problem; but it might be supposed that the rapid progress made by photography during the first ten years after its invention, together with his own experiments as to the dependence of the photographic diameter on the time of exposure, made him too sanguine that these difficulties would soon be overcome.

Since then more than twenty years have elapsed, and this hope has been realized only in a very limited degree. The photography of the fixed stars, and particularly the scientific basis of this branch of research, has made but insignificant progress since the time of Bond, as every one will agree who has had occasion to see the best recent photographs of celestial bodies. Look, for instance, at the best American photographs of nebulae, which, though excellent on the whole and most promising for the study of those bodies, yet represent the fixed stars by disks so irregular, indistinct, and large, that they are not only of no use for exact

measures, but they destroy altogether the impression of the image in their neighbourhood.

Even the latest suggestions made two years ago by the well-known French astrophysicist, M. Janssen, with the view of introducing a somewhat scientific character into photographic photometry, appear to be subject to serious objections. According to his suggestions, a photographic plate had to be exposed to the rays of the celestial body more or less rapidly, by sliding before it a screen with a triangular aperture. Admitting that the intensity of the image on the plate depends upon the time of exposure, and considering that this time must necessarily vary with the distance from the vertex of the triangle, it might be concluded that the photographic image will present a continuous series of shadings, according to the duration of the horizontal transits of the different parts of the triangle. After having received the photographic images of two different objects on two plates prepared as similarly as possible, M. Janssen proposes to find out, by direct comparison, at what distances from the base of the triangle the shadings of the two images show the same intensity. From these distances the relative times of exposure, and thence the light-ratio of the two objects, might be deduced.

Though in appearance this method satisfies in principle all photometric demands, it tacitly admits a perfect knowledge concerning the following two points:—1st, concerning the law by which the intensity of the image depends upon the intensity of the observed object. (M. Janssen has already pointed out himself that the intensity of the image is not exactly proportional to that of the object, but increases less rapidly. He therefore proposes to correct this deficiency of proportionality, by making the sides of the triangular aperture not rectilinear but slightly curved.) 2nd, concerning the relation between the light-intensity of the image and the duration of exposure, which quantities cannot be admitted, without further proof, to be exactly proportional to one another. In order that the proposed method may furnish results of real scientific value, it will therefore be necessary that the two points above mentioned should be thoroughly examined and the respective laws be established upon a sound basis.

The difficulties of the problem are still further increased by another circumstance, which, in the present state of our knowledge, can hardly be made the object of a satisfactory inquiry. It is well known that the chemical rays used in photography are very different from those which constitute the optical image, and that stars of exactly the same appearance frequently exhibit quite different spectra. On this account it must be admitted that two stars, though appearing to the eye as of equal intensity and colour, may yet be of such different physical constitution that their photographic images, from equal exposures, will be of very different intensities. Of course this argument applies in a yet higher degree to stars of different colours.

The photographic determination of the brightness of stars would therefore probably furnish us with quite a different aspect of the heavens from that given by direct ocular inspection; and this remark relates as well to coloured stars as to those which we estimate by the eye to be quite white.

Recently, at the meeting of the Royal Astronomical Society on June 8, Prof. E. C. Pickering, who has rendered such eminent services to stellar photometry, reported on a work undertaken by himself and Mr. W. H. Pickering, in which he intends to photograph all the stars visible to the naked eye, taking large sections of the heavens at once. The brightness of the stars, deduced from measures of the diameters of their photographic images, are to be compared with the results of the direct photometric measures made at Harvard College Observatory with Prof. Pickering's excellent photometers; it is to be expected stars which are more or less coloured will show the greatest differences between the results by both methods. Exact results for the brightness and the colours of the stars cannot, in my opinion, be found in this way; but the comparison of the results obtained by both methods (at present, however, of very different scientific value) may be of great service, especially in the case of great differences, in indicating the direction of researches, which may give us, in the future, very interesting information as to the physical constitution of the stars.

Pulkowa, 1883, Sept.

EDW. LINDEMANN.

The Constant of Aberration.

M. MAGNUS NYRÉN has published, in the Memoirs of the St. Petersburg Academy, a valuable paper on the determination of this important astronomical constant. Various determinations of the value of the constant of aberration made by different observers, with different instruments and by different methods, have gradually led astronomers to consider that there is a probability, or, at all events, a possibility, that W. Struve's value, notwithstanding the small probable error found for it, and notwithstanding the great care and skill bestowed on the observations and on their reduction, may be several hundredths of a second too small. M. Nyrén points out two possible sources of error, viz. the assumed regularity of rate of the clock used for observing the transits and the assumed constancy of the azimuth of the horizontal axis of the instrument in the interval between the transits of the same star East and West. Struve himself discussed these sources of error some years after the publication of his memoir, and was induced to alter his definitive value of the constant of aberration to $20''.463$ with probable error $\pm 0''.017$. (It is strange that Struve's first value $20''.445 \pm 0''.011$ has universally been adopted, instead of this corrected value.) M. Nyrén also discusses an objection to the methods which have been adopted at Pulkowa for finding the

aberration, raised by M. Yvon Villarceau on the grounds that they take no account of the absolute motion of translation of the Solar System, pointing out that on any reasonable hypothesis as to the velocity of this motion of translation the effect on the value of the constant of aberration would be probably quite inappreciable in the present state of the art of astronomical observation.

The observations which form the materials for the determination in the memoir under consideration were made by M. Nyrén with the same instrument as that used by Struve, the transit in the prime vertical, and reduced and discussed generally in the same manner. The clock used is an excellent one by Dent, and, from the attention which has been given to the matter, there is hardly a possibility of error creeping into the result from any error in its assumed rate. With regard to the azimuth of the horizontal axis of the instrument, two azimuth-marks have been set up, and frequent determinations of this element made, and every care taken to ensure accuracy, so that the present series of observations is probably free from any error arising from this source. The number of stars observed is 24 (comparing favourably with Struve's 7), very advantageously situated, having regard to the object in view; and the observations extended from Dec. 1879 to Jan. 1882, thus embracing two maxima and two minima of the aberration for each star. The final value found for the constant of aberration is

$$20''.517 + 0.20\pi \pm 0''.014,$$

where π is the mean parallax. This latter quantity comes out positive for 10 stars and negative for 14; its mean value is

$$+0''.002 \pm 0''.026,$$

so that its effect on the deduced constant is quite insignificant. M. Nyrén next proceeds to discuss certain observations of α Ursæ Minoris, δ Ursæ Minoris, and Cephei 51 Hev., made by M. Wagner during the years 1861-72. These are observations of transits taken, some by the eye-and-ear method and some by registration on a chronograph, and their discussion gives $20''.483 \pm 0''.012$ for the value of the constant of aberration.

Bringing together, then, the different values of this constant which have been found at Pulkowa from time to time, we have:—

- | | |
|---|--------------------------|
| 1. From declination observations of α Ursæ Minoris | } $20''.495 \pm 0''.013$ |
| 2. From R.A. observations of polar stars . | |
| 3. From observations in the prime vertical | $20''.490 \pm 0''.011$ |

and giving the same weight to these three mean values, the definitive value of the constant of aberration is $20''.492 \pm 0''.006$. This must be an extremely accurate value of this important constant, and will probably have to be considered final until it can be corrected by an equally accurate and extensive series of determinations

made in the southern hemisphere. Such a determination is, at the present time, a desideratum in astronomy. In combination with Cornu's determination of the velocity of light the above gives $8''.777$ for the solar parallax, whilst, if Michelson's determination be adopted, it gives $8''.791$ —a striking confirmation of the value of the solar parallax found by Mr. Gill from his heliometer observations of Mars made at Ascension in 1877.

A. M. W. DOWNING.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Changes on Jupiter.

SIR,—

I had noticed Prof. Hough's statement respecting an alleged "drift" of the great southern equatorial belt towards the red spot, and am glad that Mr. Williams has shown the change was brought about by the formation of a new belt immediately outlying the S. belt of 1880. Observers who have watched Jupiter with any closeness during the last few oppositions will have noticed the gradual development of this interesting feature, and the singular fact that it has apparently sprung from a source N. of the *f.* end of the red spot, and that it unites with the old S. belt in the same longitude as the ends of the spot. Prof. Hough's remark would lead one to suppose that the belt had shifted bodily towards the spot and partially surrounded it, which is directly opposed to the observed facts.

Mr. Williams mentions the "curious curved streak," which was the forerunner of the new belt, as being noticed in the opposition of 1881-82. It was, however, well observed in the previous year, though it seems to have received a check. Mr. F. C. Denuett describes it as "a fine [*i. e.* narrow] belt branching out from the S. equatorial belt N. of the *f.* end of the red spot, seen first on September 5, 1880, at $13^h 50^m$ " ('Observatory,' February 1881, p. 57). Mr. Barnard refers to it as "a thin reddish line *n. f.* the red spot," September 30, 1880 (see sketch 'English Mechanic,' January 14, 1881, p. 446). Prof. Hough also delineated it on September 9, 1880, at 11^h (Report of Dearborn Observatory, 1882). In succeeding months this little belt became less conspicuous, and it was described on December 24 as "a grey pencil line fading towards the equatorial belts." It reappeared, however, with greater prominence in September 1881, and since then has developed into a very intense red belt running along the S. border of the old belt and parallel to it except at the extremities of the red spot, which it curiously avoids, dipping to N., where the two belts coalesce.

Prof. Hough reiterated his opinion, before the American Association, that "the changes on the planet are not sudden but gradual,

and that many features are permanent, and attributes reports of sudden and great changes to the differences in the presentation of the markings caused by the rapid rotation of the planet." I mentioned my reasons for entertaining an entirely opposite view, in the 'Observatory' for December 1882. Prof. Hough's explanation is altogether inadequate to account for the phenomena observed. Only very inexperienced observers could be led to ascribe real changes to objects simply affected by the rapid rotatory motion of the planet, and I was not aware that any reliable reports of sudden changes had their foundation on such testimony.

The appearances of the definite markings change most palpably in very short intervals, owing to rotation, and observers who make them objects of study are, at the outset, very liable to be misled; hence we have occasional records of stupendous changes occurring in incredibly short times, but such descriptions generally bear their own condemnation and are rejected accordingly. Prof. Hough will have to seek another theory to account for the singularly rapid variation in some of the markings observed during recent years. I believe that in certain cases the different rotation periods of the spots originate a source whence real changes have been too hastily inferred; but if we disintegrate the evidence before us, and rely only upon what is rendered conclusive by unequivocal and corroborative testimony, we must perforce allow that surprising changes occur in the appearance of the markings. Some of these markings may be fairly permanent, it is true, and not liable to rapid variations, as, for example, the red spot; others, of temporary character, may be affected in different degree: it should now be our endeavour to discover the character and origin of these phenomena. The direct evidence of our telescopes warrants the conclusion that the atmospheric envelopes of this planet are in a state of great disturbance, especially in the equatorial regions, and furrowed by a remarkable series of longitudinal currents, in which rapid changes are evident.

I am, Sir,

Yours faithfully,

W. F. DENNING.

Bristol, 1883, November 3.

P.S.—With regard to Mr. Corder's query ('Observatory,' Nov., p. 339), I observed Jupiter on the morning of October 15. At 3^h 55^m the only satellites visible were the third and fourth, hanging upon the E. and W. limbs of the planet. At 4^h the fourth had completed its egress and was obviously much in advance of the time given in the 'Nautical Almanac.' The predicted disappearance of the satellites was therefore not observed, for as the third entered upon the E. limb the fourth passed off the opposite side. In fact the satellites were not at any time all included within the margin of the disk. The third satellite traversed the great S. belt as a dark spot nearly as conspicuous and black as its shadow usually appears.—W. F. D.

Magnetic Disturbance on 1883, Aug. 1.

SIR,—

In the November number of the 'Observatory,' which has just appeared, Prof. Young states, p. 330, that on Aug. 1, 1883, at 2^h 58^m local time (= 7^h 57^m Greenwich M.T.), he observed some remarkable chromospheric phenomena, and he adds that it would be interesting to ascertain whether these coincided with any magnetic disturbance. It may, I think, be worth while to point out that, according to the 'Bulletin de l'Observatoire Royal de Bruxelles,' the declination magnet in that establishment was perturbed from July 30 to August 2; and that on August 1, in the evening, towards 11^h 30^m (Greenwich M.T.), it was deflected about 14' towards the east.

Louvain,
1883, Nov. 6.

I am, dear Sir, yours faithfully,
F. TERBY.

[At Greenwich all the elements were in a disturbed state from July 29, 11^h +, to August 1, 20^h. There was some magnetic movement about Aug. 1, 11^h 30^m, but not of marked character, more activity having been shown during the two previous days. There was nothing remarkable at August 1, 7^h 57^m.—EDITOR.]

Biela's Comet.

SIR,—

In addition to the citations by Mr. Ledger of accounts of the first observation of the double nucleus of Biela's Comet by Messrs. Herrick and Bradley on the 29th of Dec. 1845, I would refer to the letter of Mr. Herrick to Prof. Mitchel published in the 'Sidereal Messenger,' vol. i. no. 6, p. 45. In that letter Mr. Herrick gives a detailed statement of their observations.

Yours very truly,
H. A. NEWTON.

Yale College,
New Haven, Connecticut,
1883, Sept. 12.

[Prof. Newton has kindly forwarded an exact transcription of the letter of Herrick referred to.]

Biela's Double Comet.

(Extracts from a letter to the Editor, from E. C. Herrick, Esq., of [the] Yale College [Observatory], dated, New Haven, Conn., Oct. 15, 1846.)

"In your notice of Biela's Double Comet, you do not refer to the observations made at Yale College Observatory, and published in the 'American Journal of Science,' N. S., vol. i. pp. 293, 446. Trusting that you will deem them worth inserting in the 'Sidereal Messenger,' I will repeat the facts more in detail.

"Through the New York Journal of Commerce we learned, December 27 1845, that Biela's Comet had been seen on the first day of that month by Prof. Challis, of Cambridge, Eng.

[On the Monday night following (Dec. 29), the sky being exceedingly clear, and the Moon absent*,] "Mr. Francis Bradley (Assistant in Practical Astronomy in the College) and myself looked for the comet and found it in a few minutes. It was much brighter than we expected to see it. Two members of the Senior Class, (Messrs. J. H. Lane and S. T. Richards,) students in Prac. Astron., were with us in the Observatory, and we examined the comet attentively for perhaps an hour and a half. We all agreed that the comet was elongated to the left and downward (the eye-piece inverting), and were surprised at the anomalous position of the supposed tail, being directed more than 90 degrees from its proper place according to the common theory. Within this supposed tail was detected a spot brighter than the general nebulosity, yet very inconspicuous compared with what we called the nucleus. It was at the time spoken of by one of the company as a *knot in the tail*. The longest diameter of the whole nebulosity was estimated at about four minutes of arc. When about the middle of January, the companion became plainly visible, it was evident that the elongation or supposed tail, observed Dec. 29th, was occasioned by the presence of the secondary comet, then just detaching itself from the principal. Wishing, subsequently, to ascertain the angle of position of the line joining the nucleus and the spot on the 29th of December, Mr. B. and myself made independent diagrams of the field, from memory; and the mean of two determinations thus obtained (which differed only 5°) was 310°, reckoning N. f. S. Although the singular direction of the supposed tail, and its strange appearance, excited our curiosity, it seemed not advisable to publish then anything on the subject, inasmuch as the comet had already been visible almost a month, and had doubtless been examined with instruments much more powerful than ours, and its peculiarities already discovered and announced. This consideration rendered us less zealous to observe the comet critically during the first half of January, especially as the clouds and the moonlight during that time made the examination of such objects very irksome and unsatisfactory. In this period we saw the comet three or four times, and it presented the same general appearance as on the 29th of December, but the moonlight permitted nothing additional to be detected. Very soon after our observation of the 29th of December, I suggested to Mr. Bradley the expediency of sending to our mutual friend, Prof. Joseph S. Hubbard of the Observatory at Washington City, a letter of inquiry as to the appearance of the comet in their Equatorial, which commands much more light than our instrument. Owing to pressure of business this letter was delayed until the suggestion was two or three times repeated, when at length it was forwarded. Two letters were written by Mr. B., the first of which spoke of the new phenomenon in more decisive terms, but through excessive caution, a less positive communication was sent in its place. The statements contained in it are based

* Note in Mr. Herrick's handwriting.

chiefly on the first observation (Dec. 29), and had it been sent on the 5th instead of the 15th of January, its tenor would have been the same; the estimate which it gives of the distance between the bodies is too large; from 1' to 2' would probably be more exact. Mr. Bradley's *original* letter (returned by Prof. Hubbard, by request,) now lies before me, dated and *post-marked* New Haven, January 15, 1846, four days before Lieut. Maury's announcement came to our knowledge. I quote from it, verbatim, the two following paragraphs relating to the comet:—

“DEAR SIR,—I wish to make an inquiry respecting Biela's Comet. It has a very singular appearance in our telescope. There is a small nebulous spot accompanying it, about 2' or 3' distant from and preceding it.

“We cannot account for it, and are anxious to know whether larger telescopes will not reveal its nature and office. You have three or four times the light on this subject that we have, and we should be pleased to have you inform us so far as you are able.”

“Lieut. Maury's announcement was made in ‘*the Union*’ of Thursday evening, January 15th, the day on which the letter above cited left New Haven.

“Had we not supposed, on the 29th of December, that any discovery which we could make in regard to the comet had been already anticipated abroad, we should have followed up the observations with more care; and it now seems wholly unaccountable, unless the sky throughout Europe was overcast from December 25 to January 14, how the companion of Biela's comet could have passed through that period undetected. It is also surprising that most of the European observers did not discover the companion until days and even weeks after it was seen in this country. At Koenigsberg, M. Widemann (Wichmann?) observed the comet with the heliometer, Jan. 14, and saw nothing of the companion, but he detected it the next night. At Cambridge, Eng., Prof. Challis first saw the companion Jan. 23 (*L., E., & D. Phil. Mag.* xxviii. 238), or else Jan. 15 (*Comptes Rendus*, xxii. 333). On the 18th of January this secondary comet, as seen here, had become quite conspicuous, so as to be obvious at the first glance, and yet M. Valz at Marseilles, says, “les 18 et 20 janvier, la comète n'offre rien de particulier.” After a cloudy period he discovered the companion, Jan. 27, on which day MM. D'Arrest and Encke also saw it.

“The final observations made at Yale College on the comet and companion, you will see at p. 446, vol. i. of the *Journal of Science* before cited. Perhaps you may think them worth copying as an appendix to the foregoing communication. The southern or principal comet was very faint on the 16th of April, but perfectly evident. It was not looked for by us after this date. In favourable circumstances it might have continued visible in our instrument probably a week longer.

“Very respectfully,

“Your ob't serv't,

“EDW'D C. HERRICK.”

NOTES.

RATES OF MOTION IN THE SUN.—An interesting controversy has been carried on recently between MM. Faye and Thollon on the interpretation of the displacement of lines in the solar spectrum, M. Faye rejecting as fabulous the movements of 300 or 400 miles per second which seem to be indicated by the rapidity with which certain prominences seem to recede from the Sun, and by the displacements observed in the hydrogen lines. M. Thollon replies that when the differences of density, composition, and temperature are borne in mind these movements are not more incredible in solar cyclones than movements of 90^m or 100^m in terrestrial cyclones. Further, the movements inferred from the formation of prominences and from the displacement of lines are quantities of the same order, and their maximum rapidity corresponds with that of cometary bodies near the Sun. And since motion in the line of sight undoubtedly produces a displacement in the spectral lines, it is only natural, in the absence of any other known cause, to ascribe such displacements to this one. M. Faye replies that we have well authenticated observations of prominences, the spectra of which showed the hydrogen lines much distorted and displaced, but the lines of other substances in their normal positions, and we should have on this hypothesis to suppose that hydrogen mixed with other gases could carry some along with it and leave others behind. With regard to vertical movements M. Faye suggests that hydrogen expelled from the Sun would become cold, and therefore invisible, through the great expansion it would undergo, but becoming heated by the solar radiation it would again become visible, and adduces Secchi's observations as showing that prominences were often formed by a local heating or by some other agitation of hydrogen already present and not by actual transport of matter from a distance. M. Thollon, however, points out, first, that the lines in the spectrum of a prominence differ exceedingly in brilliancy, and that our inability to see displacements in the feebler lines is no proof that they have not occurred. Prof. Young has mentioned that he has never seen the 1474 K line displaced, but M. Thollon under very favourable circumstances has been able to observe displacements in it. It is needful to be very cautious in spectroscopic work not to assume that that which is invisible does not exist. Thus in the chromospheric spectrum only eight lines can be usually seen, but at the summit of the Pic du Midi, M. Thollon could make out 40, and in a total eclipse hundreds have been seen. So again it does not seem admissible that the mixture of gases and vapours in a prominence as large as or much larger than the Earth should be perfect. Besides in looking at the limb of the Sun we are looking for a depth of about 300,000 km. through the region of the prominences, and all the masses of glowing gas that may lie at

different points along this line will seem to us as forming but one single prominence. M. Faye's objection therefore that one part of the prominence could not be at rest whilst the remainder of it was in motion falls to the ground, and the only solid objection to the generally received interpretation lies in the greatness of the speeds involved.

VARIATIONS IN THE LIGHT OF COMET 1883 *b* (PONS-BROOKS).—Fuller details of the observations made of this comet on the nights of Sept. 21, 22, and 23 prove its remarkable change of appearance to have taken place in a surprisingly short space of time. On Sept. 21 Mr. Chandler, observing at Harvard College, described the comet as "very faint and diffuse. Central condensation or nucleus very faint, not brighter than 10 or 11 mag., about equal to 11 mag." "Nebulosity circular, $1\frac{1}{2}'$ in diameter." "The comet presented sensibly the same faint and diffuse appearance as on the 19th of Sept and previously." On the following evening, Sept. 22, Sig. Schiaparelli, observing at Milan at about 8^h 10^m Greenwich M.T., noted that "the comet was about 3' in diameter, but faint and confused, the nucleus of the 13th mag.; the sky not perfectly clear." The same evening, at about 11^h 45^m G.M.T., Mr. Chandler records that he "turned the telescope on the comet, and was astonished to find exactly in its place a bright, clearly-defined 8 or 8½ mag. star without sensible trace of nebulosity, except in low power (50 diameters, field $1\frac{1}{4}^\circ$), and even with that not noticeable, except with attention. So distinctly stellar was the object, that even an experienced observer would easily have failed to distinguish it from any of the neighbouring 8 mag. stars." Mr. Chandler's note made at the time runs as follows:—"There is a most surprising change in the appearance of the comet since last night. Instead of the diffuse nebulosity with scarcely visible nucleus, it is a bright star-like object surrounded by a faint nebulosity $\frac{3}{4}'$ diameter, much smaller than last night. Nucleus sharp and at least 2 mags. brighter, about 8½ mag." This marvellous change therefore had taken place within three and a half hours.

The comet had somewhat changed its appearance again before Sig. Schiaparelli saw it on the following evening. It then appeared as a star somewhat fainter than the 8th mag., with a very faint circular nebulosity from 1' to $1\frac{1}{4}'$ in diameter. The central mass was not a point of light, but had a sensible diameter and an ill-defined outline. This was about 9^h 0^m G.M.T. A little after Greenwich midnight Mr. Chandler again observed it, and described it in much the same terms as Sig. Schiaparelli, except that the nebulous envelope would now appear to have been considerably brighter.

The phenomenon was repeated on a small scale on Oct. 15, when a nucleus of about 9.3 mag. appeared, which in its turn gradually dissipated on the succeeding evenings by expansion into the general nebulosity.

Approximate Times of Transit of the great Red Spot and Equatorial White Spot across the Central Meridian of Jupiter in December 1883.

Day of Month.	Red Spot on C.M.	White Spot on C.M.	Day of Month.	Red Spot on C.M.	White Spot on C.M.
	h	h		h	h
1.	19.3	11.0	17.	12.4	10.5
2.	15.1	16.5	18.	8.3	16.0
3.	11.0	12.2	19.	14.1	11.7
4.	16.7	17.7	20.	9.9	17.2
5.	12.6	13.3	21.	15.7	12.9
6.	18.4	18.8	22.	11.6	18.4
7.	14.2	14.5	23.	17.3	14.0
8.	10.1	10.2	24.	13.2	9.7
9.	15.9	15.7	25.	9.0	15.2
10.	11.7	11.3	26.	14.8	10.9
11.	17.5	16.8	27.	10.7	16.4
12.	13.3	12.5	28.	16.4	12.1
13.	9.2	18.0	29.	12.3	7.8
14.	14.9	13.7	30.	8.1	13.2
15.	10.8	9.3	31.	13.9	8.9
16.	16.6	14.9	1884.		
			Jan. 1.	9.7	14.4

I observed the red spot crossing the planet's central meridian on Nov. 3 at 16^h 17^m, on Nov. 18 at 13^h 36^m, on Nov. 19 at 19^h 25^m, and on Nov. 25 at 14^h 25^m, so that it follows Mr. Marth's assumed first meridian ('Monthly Notices,' Vol. xliii. pp. 454-458) by as nearly as possible two hours. The spot continues faint, though with good definition it is seen without difficulty.

The equatorial white spot was estimated central on Nov. 3 at 14^h 17^m; but the observation was rather uncertain, the spot being very small and faint. On Nov. 25 the spot was brilliant as it came to the C.M. at 17^h 25^m.

The two objects will arrive at nearly the same longitude on Dec. 8; and it will be important to note their relative positions either on the mornings of the 8th and 10th, or evening of the 8th, so that the time of conjunction may be approximately ascertained.

The position of Jupiter has now become so favourable that two successive transits of the same markings may be obtained in one night. Thus, on Dec. 13 the red spot will be on the central meridian at 9^h 2 and again at 19^h 1, on Dec. 15 the white spot will arrive at the C.M. at 9^h 3 and again at 19^h 1, &c. It is to be hoped that, under these circumstances, more observers will apply themselves to the observation of these interesting features.

Bristol, 1883, Nov. 26.

W. F. DENNING

Ephemeris of Comet Pons-Brooks.

For Greenwich Midnight.

The following ephemeris is extracted from one given in 'Nature,'
Vol. xxix. p. 45.

1883.	R.A.	Dec.	Log Δ.	Log r.
	h m s	° ' "		
Nov. 29....	18 27 18	+46 57.9	0.0583	0.1102
Dec. 1....	18 35 31	46 25.0		
3....	18 44 51	45 49.0	0.0311	0.0923
5....	18 54 20	45 9.4		
7....	19 4 19	44 25.5	0.0023	0.0738
9....	19 14 48	43 36.5		
11....	19 25 47	42 41.5	9.9721	0.0549
13....	19 37 17	41 39.5		
15....	19 49 17	40 29.3	9.9409	0.0356
17....	20 1 47	39 9.8		
19....	20 14 45	37 39.8	9.9093	0.0161
21....	20 28 9	35 57.8		
22....	20 41 54	34 2.8	9.8784	9.9965
25....	20 55 57	31 53.6		
27....	21 10 13	29 29.3	9.8500	9.9772
29....	21 24 37	26 49.5		
31....	21 39 4	+23 54.9	9.8263	9.9585

*Differences of Right Ascension and Declination between Iapetus and
the centre of Saturn.* By A. MARTH*.

	$\alpha - A.$	$\delta - D.$		$\alpha - A.$	$\delta - D.$
		"			"
Dec. 1	- 6.4	-188	Dec. 17	+38.1	+ 50
2	- 3.1	-179	18	+39.4	+ 67
3	+ 0.3	-169	19	+40.5	+ 83
4	+ 3.7	-157	20	+41.3	+ 99
5	+ 7.1	-145	21	+41.9	+114
6	+10.4	-131	22	+42.3	+128
7	+13.6	-117	23	+42.3	+142
8	+16.8	-102	24	+42.2	+155
9	+19.8	- 86	25	+41.7	+167
10	+22.7	- 70	26	+41.1	+177
11	+25.5	- 53	27	+40.1	+187
12	+28.1	- 36	28	+39.0	+196
13	+30.5	- 19	29	+37.6	+203
14	+32.7	- 2	30	+36.0	+210
15	+34.7	+ 16	31	+34.2	+215
16	+36.5	+ 33	32	+32.2	+218

* Monthly Notices, Vol. xliii. No. 8.

Equation of Time :—Sun *before* Clock, Dec. 1, $10^m 51^s$; Dec. 11, $6^m 38^s$; Dec. 21, $1^m 47^s$; *after* Clock, Dec. 31, $3^m 9^s$.

Sidereal Time at Mean Noon :—Dec. 1, $16^h 40^m 0^s$; Dec. 11, $17^h 19^m 4^s$; Dec. 21, $17^h 58^m 9^s$; Dec. 31, $18^h 38^m 3^s$.

Moon.						rises.		
	h	m		h	m		h	m
Dec. 1..	5	38	Dec. 11..	17	29	Dec. 21..	12	51
2..	6	34	12..	18	43	22..	13	55
3..	7	37	13..	rises.		23..	14	56
4..	8	43	14..	4	49	24..	15	57
5..	9	53	15..	5	56	25..	16	58
6..	11	3	16..	7	8	26..	17	55
7..	12	17	17..	8	20	27..	18	47
8..	13	33	18..	9	31	28..	sets.	
9..	14	52	19..	10	40	29..	4	26
10..	16	11	20..	11	44	30..	5	27
						31..	6	34

First Quarter, Dec. 6, $23^h 46^m$; Full Moon, Dec. 13, $15^h 28^m$; Last Quarter, Dec. 20, $20^h 8^m$; New Moon, Dec. 29, $1^h 0^m$.

Mercury is an evening star, and may be observed after sunset in the latter part of the month.

Venus is an evening star. Diameter :—Dec. 1, $10'' 4$; Dec. 31, $11'' 2$. Illuminated portion of disk, 0.936 on Dec. 15.

Dec. 1, R.A. $17^h 46^m 9$, Dec. $24^\circ 20'$ S., tr. $1^h 7^m$, sets $4^h 54^m$
31, 20 28 9, 20 45 S., 1 51 6 2

Mars is stationary on Dec 23, 21^h . Diameter :—Dec. 1, $11'' 4$; Dec. 31, $14'' 6$. Illuminated portion of disk 0.928 on Dec. 15.

Dec. 1, R.A. $9^h 29^m 4$, Dec. $17^\circ 25'$ N., tr. $16^h 48^m$, rises $9^h 10^m$
31, 9 40 5, 17 50 N., 15 0 7 20

Jupiter is in Cancer. Diameter :—Dec. 1, $39'' 8$; Dec. 31, $42'' 6$.
Dec. 1, R.A. $8^h 27^m 3$, Dec. $19^\circ 35'$ N., tr. $15^h 45^m$, rises $7^h 54^m$
31, 8 17 7, 20 13 N., 13 37 5 42

Saturn is retrograding in Taurus.

Dec. 1, R.A. $4^h 19^m 8$, Dec. $19^\circ 25'$ N., tr. $11^h 38^m$, sets $19^h 28^m$
31, 4 10 6, 19 7 N., 9 31 17 19

Outer Ring.			Inner Ring.			Ball.
Maj. Axis.	Min. Axis.		Maj. Axis.	Min. Axis.		Diam.
Dec. 5	$46'' 35$	$19'' 97$	$30'' 82$	$13'' 28$		$18'' 4$
25	$45 77$	$19 64$	$30 44$	$13 06$		$18 2$

The south side of the rings is visible, the elevation of the Earth above their plane being $25^\circ 32'$ on Dec. 5, and $25^\circ 25'$ on Dec. 25; and of the Sun $25^\circ 50'$ and $25^\circ 55'$.

Uranus is in quadrature with the Sun, Dec. 19, 19^h .

Neptune is retrograding in Aries.

Dec. 1, R.A. $3^h 8^m 8$, Dec. $15^\circ 46'$ N., tr. $10^h 27^m$, sets $17^h 54^m$
31, 3 6 2, 15 37 N., 8 27, 15 52

Phenomena.

G. M. T.			G. M. T.		
	h	m		h	m
Dec. 1	10	45	J. i. Oc. R.	Dec. 15	11 12 J. i. Ec. D.
	11	32	J. ii. Ec. D.		14 18 J. i. Oc. R.
	16	32	J. ii. Oc. R.		15 13 68 Geminorum
3	10	49	J. ii. Tr. E.		Oc. D. 42°.
	15	22	J. iv. Tr. I.		16 45 J. ii. Ec. D.
5	18	36	J. i. Tr. I.	16	9 16 J. i. Tr. I.
6	14	50	J. i. Ec. D.		11 36 J. i. Tr. E.
	18	5	J. i. Oc. R.	17	8 44 J. i. Oc. R.
7	11	58	J. iii. Oc. R.		15 27 J. ii. Tr. E.
	15	23	J. i. Tr. E.	17	23 ω Leonis
8	9	18	J. i. Ec. D.		Oc. D. 65°.
	12	32	J. i. Oc. R.	18	12 8 16 Sextantis
	14	9	J. ii. Ec. D.		Oc. R. 241°.
	18	55	J. ii. Oc. R.	19	10 25 J. ii. Oc. R.
9	9	50	J. i. Tr. E.	20	11 5 J. iv. Tr. E.
10	10	15	J. ii. Tr. I.		18 37 J. i. Ec. D.
	13	9	J. ii. Tr. E.	21	12 35 J. iii. Ec. D.
11	15	40	J. iv. Ec. D.		18 51 J. iii. Oc. R.
12	6	10	B.A.C. 1272.		18 55 J. i. Tr. E.
			Oc. D. 50°.	22	13 5 J. i. Ec. D.
	14	36	δ ³ Tauri		16 3 J. i. Oc. R.
			Oc. D. 53°.	23	11 1 J. i. Tr. I.
13	4	42	m Tauri		13 21 J. i. Tr. E.
			Oc. D. 134°.	24	7 34 J. i. Ec. D.
	5	23	m Tauri		10 29 J. i. Oc. R.
			Oc. R. 240°.	17	44 J. ii. Tr. E.
	15	55	119 Tauri	25	7 47 J. i. Tr. E.
			Oc. D. 70°.		8 35 J. iii. Tr. E.
	16	43	J. i. Ec. D.	26	8 39 J. ii. Ec. D.
	17	24	120 Tauri		12 43 J. ii. Oc. R.
			Oc. R. 285°.	28	9 40 J. iv. Ec. D.
14	8	36	J. iii. Ec. D.		13 50 J. iv. Ec. R.
	15	27	J. iii. Oc. R.		16 33 J. iii. Ec. D.
	17	10	J. i. Tr. E.		19 10 J. iv. Oc. R.
	19	10	26 Geminorum	29	14 59 J. i. Ec. D.
			Oc. D. 105°.		17 47 J. i. Oc. R.
15	7	20	λ Geminorum	30	12 46 J. i. Tr. I.
			Oc. D. 76°.		15 6 J. i. Tr. E.
	8	14	λ Geminorum	31	9 27 J. i. Ec. D.
			Oc. R. 277°.		12 13 J. i. Oc. R.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

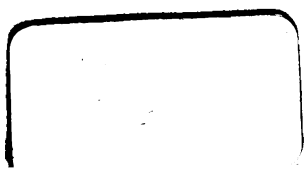
EDITOR.

ERRATA IN No. 79.

Page 315, line 5 from foot of page, for Bailey read Bailly.

349, line 1, for Nov. 1, 18^m 16^a, read Nov. 1, 16^m 18^a.

3 2044 077 071 918





3 2044 077 071 918